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THE

PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND
THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS, ERNEST MERRITT
AND FREDERICK BEDELL

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THE

PHYSICAL REVIEW.

A NEW FORM OF CATHODE DISCHARGE AND THE PRODUCTION OF X-RAYS, TOGETHER WITH SOME NOTES ON DIFFRACTION.

PRELIMINARY COMMUNICATION.

By R. W. Wood.

If accurate measurements are to be made of diffraction bands produced by the x-rays, it seems probable that very narrow slits, and a very intense and minute radiating source must be used. The focus tube has the disadvantage that the radiation is given off from a surface of considerable size, and is not very intense *per unit of area*, which is what is required for diffraction work. What we need is something that bears the same relation to the focus tube that the arc-light bears to a large flame. After considerable experimenting I have succeeded in finding a method of producing the rays by what appears to be a new form of cathode discharge, which manifests itself as a bright blue arc between two minute balls of platinum in a very high vacuum.

The advantage of the method is that a very small bulb can be used, an inch or less in diameter (of interest to those bent on getting a tube into the human body and lighting it up from within), which yields a radiation intense enough to show the bones of the forearm distinctly, from a source about the size of a small pin-head. *Per unit of area* of the radiating surface, I find the radiation to be between ten and twenty times as intense as with the best focus tubes. This statement I base on photographs made of the two sources by means of a pin-hole 0.2 mm. in diameter.

I have not yet succeeded in producing a "total radiation" as intense as the large focus tubes yield, where the source is a plate of considerable area, but the method seems more rational than that of focusing the cathode rays on a platinum target, and I think it not unlikely that the development of a very powerful tube is possible on the lines indicated in this paper.

The apparatus is shown $\frac{1}{2}$ natural size in Fig. 1. The small bulb is blown on the top of a tube 80 cm. long, which dips into a jar of mercury. Within this tube is another, bent into a U at the bottom, one arm projecting out of the mercury. A conducting wire runs through this, and is attached to a platinum wire sealed through the glass at the top.

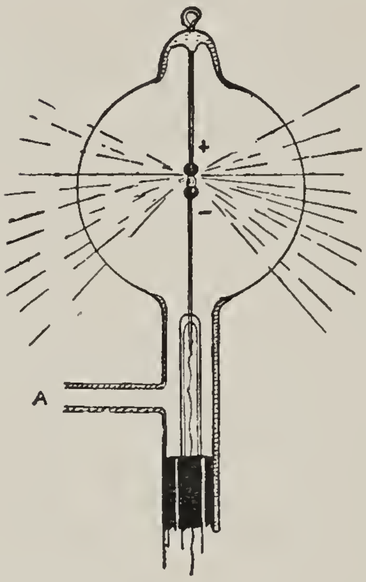

Fig. 1.

The arrangement is the same as that devised by the writer for determining the temperature variations in the stratified discharge, by means of a sliding bolometer, and will be better understood by referring to Vol. IV, No. 3, of the Physical Review (Fig. 4). This rather complicated arrangement is not necessary at all, but it facilitates adjustments within the tube, and has been used thus far.

The electrodes are two platinum wires carrying small platinum balls, about 1.5 mm. in diameter, easily made by fusing the wire in the electric arc. One is fixed and the other can be moved up and down by means of the inner tube which supports it. The bulb is exhausted through the side tube *A*, which communicates with a mercury pump.

Starting with the balls about 5 mm. apart, the tube is exhausted (first clamping the projecting arm of the U) until, on passing a discharge through it, the green luminescence due to the cathode rays is very pronounced. The balls can be pushed within a millimeter of each other without producing any apparent change, the cathode rays being given off from the entire surface of the wire and ball.

If now a large spark gap be introduced into the external circuit, the green luminescence disappears as does also the bluish negative light, and the discharge passes between the balls in the form of a minute and brilliant blue arc. I use the word "arc" because on close inspection the discharge resembles an arc rather than a spark. It is intermittent of course. From the surface of the anode ball on which this arc plays, the *x*-rays emanate with great intensity.

I have been using a large 12-plate Wimshurst machine, which seems to work better than a coil. A good deal depends on the conditions: the platinum balls must be adjusted to the maximum striking distance (usually about 1 mm.). If they are too close together, the radiation is feeble. The external spark gap must also be adjusted carefully: if it is too short or too long, the arc disappears, we get the usual diffuse cathode rays, and the *x*-radiation vanishes except for the feeble effect from the glass.

When everything is just right, the little arc burns away steadily without interruptions, except the rapid periodic ones, and the anode radiates precisely as the positive carbon in the arc-light.

Another rather curious analogy between the discharge and the carbon arc is that the anode ball is eaten away to a shallow crater, while on the surface of the cathode is built up a dense, hard deposit of very brilliant metallic platinum. The appearance of the balls under the microscope, after a five-hour passage of the arc, is shown in Fig. 2. They are not visibly heated by the discharge, though at times showers of red-hot sparks are given off, which bounce about inside the bulb, and pour down into the barometer tube. The surface of the anode, where it has been eaten away, is pitted and corrugated but very brilliant: the built-up mass on the cathode has a similar appearance, but the rest of the surface is dull and without luster.

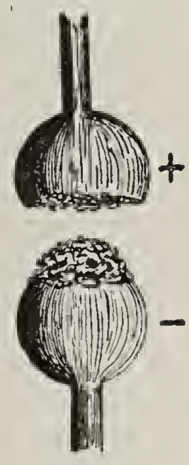

Fig. 2.

Another curious action of the arc is, that if it be made to play on the edge of a piece of platinum foil (used as anode), the edge is sharply crimped or turned up towards the cathode. In one tube in which the radiating anode was a small horizontal plate of platinum 0.5 cm. square, on the center of which the arc played, the plate was bent into an arc of perhaps 60° concave towards the cathode. Although there is a strong attractive force between the anode and cathode (shown by the flying together of the balls if they are side by side), it does not seem to be sufficient to explain this action: moreover, the plate is not heated even to redness. The effect is such as would be produced if the surface on which the arc played was put in a state of tension, but as this surface is the one that is continually cut away by the discharge, it is difficult to imagine how such a state could be produced. A similar bending or crimping of the anode sometimes takes place in focus tubes.

I have already alluded to the spark showers which sometimes take place within the tube. On one occasion after running the current in one direction for several hours, I reversed it suddenly and observed a phenomenon which I have never seen surpassed in point of beauty in any of the various forms of vacuum tubes. From the anode there shot out two intense yellow beams resembling those of search lights shining through a mist (Fig. 3). The edges of the beams were as sharply defined as I have indicated in the drawing, and the closest scrutiny showed them structureless. Where they struck the wall of the bulb they apparently splashed off, and were somewhat brighter than at points midway between the source and the wall. The bulb was about 10 cm. in diameter, and the electrodes were shaped like those in Fig. 8, which will be alluded to later. The appearance of the beams was unlike anything that I have ever seen in a vacuum tube, and the only explanation that I could think of was that they might be caused by minute particles of heated platinum thrown off from the anode, too small to be perceived individually by the eye, but collectively presenting the appearance of a ray of light in smoky air. Subsequent events showed that beyond a doubt this was the

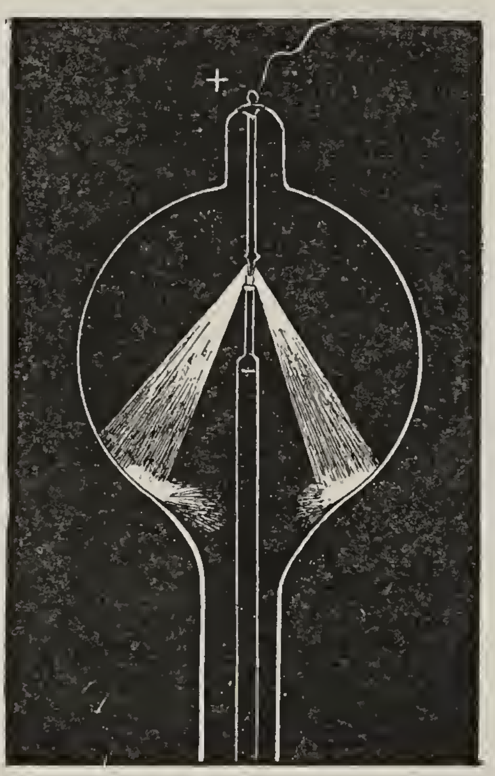

Fig. 3.

true explanation, for in a few minutes a close examination revealed the presence of bright lines in the rays which became more and more distinct, until no doubt remained that the yellow beams were in reality blasts of red-hot metallic particles which, at the outset, may have been of nearly molecular dimensions.

The phenomenon is undoubtedly caused by the rapid cutting away of the built-up deposit on the electrode to which I have already alluded, as I have observed it only when that electrode which has served for some time as cathode is suddenly made anode. The deposit built up by the arc, though apparently as hard and solid as the rest of the ball, is probably a dense aggregation of very minute particles, which is rapidly undermined and torn to pieces when the current is reversed. As we get deeper down into the mass, the projected particles become larger, until their individual trajectories can be seen; and the luminous beams become transformed into the common spark shower.

The behavior of these beams in a powerful magnetic field may be worth investigating, though it is doubtful if any effect would be found.

To return now to the tube shown in Fig. 1. By means of the pin-hole I photographed the radiating surface on a plate wrapped up in several thicknesses of black paper and obtained an image which is shown slightly magnified in Fig. 4. It will be seen that the under surface of the ball gives out most of the rays, while a few come from the wire: this, together with the fact that the extreme top of the bulb shines with a green light, leads me to think that the little arc is a very dense bundle of cathode rays, a few of which get around the ball and impinge, some on the wire and some on the glass. I have also noticed that particles of aluminum oxide on the upper surface of the anode ball, where no rectilinear rays from the cathode could possibly reach them, glow with a rich red color, while the arc plays between the balls, a fact which can only be explained by supposing that the cathode rays curve around and envelop the + electrode in the vicinity of the arc. If the balls are pushed nearer together, an aureole a centimeter or more in diameter surrounds the arc, which is bluish white with

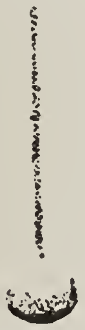
Fig. 4.

platinum electrodes and bright green with copper. There are a number of points about the arc that require investigation. I find that it can be produced to a less degree between the cathode and a metal plate insulated on a glass stem, the anode being below. This suggests that oscillations may be set up, and it may be possible to construct a vibrator for short waves in some such way. The spectrum of the arc and its behavior in the magnetic field are also worth investigating. Most of the light comes from a film on the anode, which is sometimes yellowish, though usually blue. A more complete study of the nature of the discharge and the distribution of the cathode rays about it will be made in the near future.

Reference to Fig. 4 shows us (considering that the ball is but 1.5 mm. in diameter) that the chief source of the rays is an exceedingly narrow horizontal line, especially if the ball has been somewhat cut away, and we might therefore expect some diffraction phenomena to show themselves on the edges of shadows of objects thrown by such a source. This I find to be apparently the case. If a piece of fine copper wire 0.2 mm. in diameter be fastened horizontally on the outside of the bulb opposite the balls, and another wire vertically, making a +, the horizontal wire, which is parallel to the narrow radiating source, throws a sharp shadow on a plate 40 cm. away, while the vertical wire throws scarcely any shadow at all.

The shadow of the wire, which is of course light on the photographic plate, has a dark border, darker than the rest of the surface which receives the whole radiation, and there seems no other way of explaining this increased blackening on the edge of the shadow than by interference. Shadows of a wire mounted within the tube also showed this dark border. The distance of the wire from the radiating source was about 1.5 cm., and the plate was about 40 cm. from the wire, so that the shadow of the wire was nearly half a centimeter wide, a magnification of about 20 diameters: in spite of this it was in most cases sharply defined.

The dark border was only to be found *on one edge;* namely, the one towards the radiating anode. The other edge appeared slightly lighter than the rest of the shadow, somewhat as indicated in Fig. 5. If the upper ball was the anode, the dark border was on the upper side of the shadow; if, however, the lower ball was

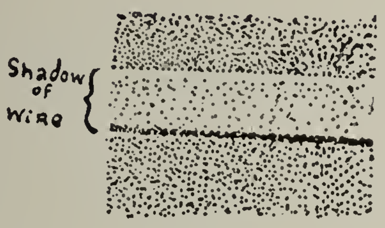

Fig. 5.

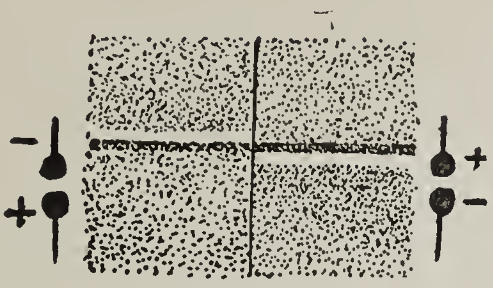

Fig. 6.

made anode by reversing the current, the light and shade were reversed. (Fig. 6: both exposures made on the same plate, one half of it being screened each time with a strip of lead.)

I see no way of explaining these effects except by diffraction or something analogous to it, for no half-shadows or penumbræ will account for the edge of the wire's shadow being darker than the rest of the plate which is exposed to the full action of the rays. There seemed at first sight to be no way of explaining the difference in the appearance of the two edges of the shadow; or in other words, why the edge of the wire towards the radiating surface should act differently from the one facing away from it. Some rotational element in the rays, such as would exist if they were vortex filaments in the plane of the wire, might cause them to be affected differently by the two edges, but such an explanation is hardly worth considering, at least on present evidence.

By combining diffraction with penumbral effects we might possibly get a similar result: reference to Fig. 4 shows that the radiation, though most intense from the under side of the ball, comes also to a less degree from the sides as the arc plays about over the surface.

Draw a figure showing the paths of two pencils of rays; a strong one from the under surface and a weaker one from a point above, and suppose the two edges of the wire's shadow thrown by the first pencil to have dark edges due to interference maxima. It will be seen that the feebler rays coming from the upper source will cast a shadow of the wire a trifle lower down than the first shadow, but will illuminate all the rest of the plate. If this shadow falls on the under dark band, it will tend to obliterate it, if the relative intensities are about equal; that is, if the darkening due to the rays from the upper source is about equal to the increased darkening due to interference. If the lower ball is made anode, the conditions are reversed, the feebler source being below the brighter, and we have the maximum on the upper edge of the shadow obliterated.

To remove all chances of penumbræ a much narrower radiating source is necessary, and I made a great number of experiments to determine the best possible form to give the apparatus. I first tried a long narrow bulb with the platinum beads very close to the glass, and a slit .05 mm. wide fastened with wax to the outside just opposite the anode. This form, though giving excellent results at first, was very short lived and very apt to be punctured by the discharge. Better results were obtained with a form shown in Fig. 7. Over the mouth of a thistle tube was fastened with sealing wax a sheet of very thin aluminum plate, which served as cathode. The anode was a platinum bead which is pushed up until the arc plays between the plate and the ball; the *x*-rays coming from the ball pass through the aluminum plate, on which is fastened a narrow slit of copper or platinum. The chief advantage of this form is that the cutting away of the anode does not alter its position with reference to the slit, though the deposit of platinum on the aluminum plate soon lessens its transparency.

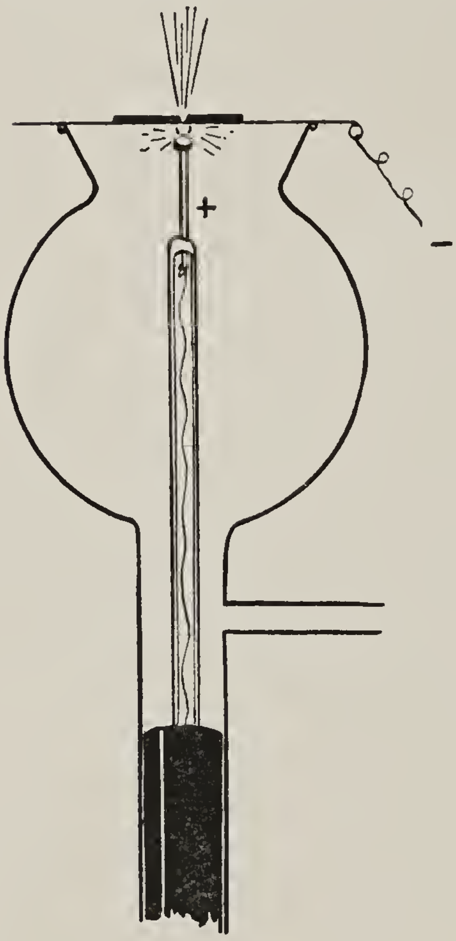

Fig. 7.

The best form found thus far is shown in Fig. 8. A rectangular plate of platinum foil is partially cut across with the point of a very sharp penknife: this makes a slit about 0.05 mm. wide with very uniform straight edges. The plate is soldered with gold to the wire carrying the cathode, which is a short piece of thick plati-

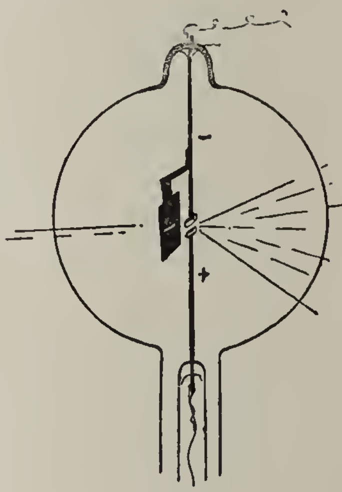

Fig. 8.

num wire mounted parallel to the slit. The anode is of similar form and is mounted on the movable rod, and the arc plays back and forth between the two cylindrical surfaces.

The diffraction slit, furnished with a micrometer screw, is placed at a distance of about 15 cm., as nearly as possible in the center of the beam which comes through the knife cut in the platinum plate (the proper position being found by means of a small screen of calcium tungstate), and the photographic plate behind this, at a distance of 25 cm. It is important that the electrode carrying the plate be made cathode, otherwise *x*-rays are given off in profusion from both its surfaces as shown by a pin-hole photograph.

I tried one other arrangement which promises to be even better than this, in that it furnishes radiations from a source 0.05 mm. in diameter without the use of a slit, which makes the position of the diffraction slit immaterial, providing it be only parallel to the source. This method is to allow the cathode arc to play upon a horizontal and tightly stretched bit of platinum wire 0.05 mm. in diameter, and gives beautiful definition while it lasts, but unfortunately the discharge cuts through the wire in ten or fifteen minutes.

I have designed a tube in which new wire can be continually fed to the arc, which I think may be better adapted to diffraction work than the one with the slit.

The images of the slits on the photographic plate give evidence of a maximum darkening on each edge. With a comparatively wide slit (0.2 mm.) the appearance is about as indicated in Fig. 9 (magnified). This was obtained with the plate at a distance of about 15 cm. from the slit. With a slit 0.05 mm. wide at a distance of 25 cm. the image was uniformly dark, but there was a very faint trace of a light border followed by a faint shadow. This looked more like true diffraction than any of the other results, and for comparison a plate was made under similar conditions with light. A rough calculation based on these two plates would indicate a wave length of about 0.00004 for the *x*-rays, a trifle less than one half that of Schumann's shortest light wave. I place very little confidence in this estimate, and in fact am not yet at all

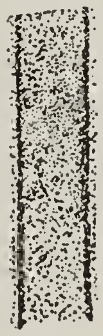
Fig. 9.

sure that we are dealing with true diffraction. There seems to be very strong evidence, however, that the rays do not pass by the edges of obstacles in rectilinear paths without suffering any change, although there remains a good deal of work to be done before any very definite statements as to just what happens can be made.

It does not seem to me to be unreasonable to suppose that there may be some action which is not identical with the interference of light and heat waves.

In conclusion, I wish to express my thanks to Professor Charles R. Cross for his great kindness in placing at my disposal the facilities of the Rogers Laboratory of Physics of the Massachusetts Institute of Technology.

Jamaica Plain, March 27, 1897.

AN EXPERIMENTAL STUDY OF INDUCTION PHENOMENA IN ALTERNATING CURRENT CIRCUITS.

By F. E. Millis.

2. *Resonance in Alternating Current Circuits.*

(Concluded from Vol. IV., Page 142.)

IN a circuit containing an electromotive force, resistance, self-induction, and capacity, the equation by which the current may be computed from the electromotive force is

$$e = Ri + L\frac{di}{dt} + \frac{q}{c}, \tag{8}$$

in which e is the impressed electromotive force; R, the resistance; L, the self-induction; C, the capacity; i, the current at any instant of time t; and q, the charge of the circuit at any instant.

Ri represents the electromotive force necessary to overcome the ohmic resistance; $L\frac{di}{dt}$, that necessary to overcome the counter-electromotive force of self-induction; and $\frac{q}{c}$, that necessary to overcome the counter-electromotive force due to the capacity of the circuit; and their sum equals the impressed electromotive force e. If the impressed electromotive force is periodic, and a single-valued function of the time, it may be represented by $e = f(t)$.

This may be written by Fourier's theorem:

$$e = A_0 + \sum_{n=1}^{n=\infty}(A_n \sin n\omega t + B_n \cos n\omega t), \tag{9}$$

where n takes all plus integer values; $\omega = 2\frac{\pi}{\tau}$, τ being the time of one period; t the time at any instant counted from the beginning of the wave, and A_1, A_2, etc., B_1, B_2, etc., are constants to be determined. A_0 is the average algebraic sum of the ordinates, and will equal zero if the line which divides the curves so that the areas of the negative waves shall be equal to the areas of the positive waves, be taken as the zero line from which to measure the ordinates.

Any two waves having the same period, and of the forms $y_1 = a \sin \omega t$ and $y_2 = b \cos \omega t$, may be compounded into the single sine wave $Y = K \sin (\omega t + \phi)$, where $K = \sqrt{A^2 + B^2}$, and $\tan \phi = \frac{B}{A}$, since the components are at right angles. Hence equation (9) may be written:

$$e = \sum_{n=1}^{n=\infty} K_n \sin (n\omega t + \phi_n) = f(t). \tag{10}$$

With this expression for the electromotive force, we obtain from (8) the following expression for the current:

$$i = \sum_{n=1}^{n=\infty} \frac{Kn}{\sqrt{R^2 + \left(\frac{1}{n\omega C} - n\omega L\right)^2}} \sin \left[n\omega t + \phi_n + \tan^{-1}\left(\frac{1}{n\omega CR} - \frac{n\omega L}{R}\right)\right] + k_1 \epsilon^{-\frac{t}{H_1}} + k_2 \epsilon^{-\frac{t}{H_2}}, \tag{11}$$

which is a general expression for the periodic current flowing in a circuit, there being as many terms in the summation as there are in the impressed electromotive force.

Here k_1 and k_2 are integration constants, ϵ is the Naperian base, $H_1 = \frac{2LC}{RC - \sqrt{R^2C^2 - CL}}$, and $H_2 = \frac{2LC}{RC + \sqrt{R^2C^2 - CL}}$.

The positive and negative waves of electromotive force are usually symmetrical, hence only odd harmonics will be present. Usually only the third or fifth harmonic will be appreciable; hence

$$\begin{aligned} e = A_0 + A_1 \sin \omega t + B_1 \cos \omega t + A_3 \sin 3\,\omega t + B_3 \cos 3\,\omega t \\ + A_5 \sin 5\,\omega t + B_5 \cos 5\,\omega t \end{aligned} \tag{12}$$

may be taken as the complete expression for the impressed electromotive force. This equation may be written by (10).

$$e = A_0 + K_1 \sin (\omega t + \phi_1) + K_3 \sin (3\,\omega t + \phi_3) + K_5 \sin (5\,\omega t + \phi_5), \tag{13}$$

in which

$$K_1 = \sqrt{A_1^2 + B_1^2},\ K_3 = \sqrt{A_3^2 + B_3^2},\ K_5 = \sqrt{A_5^2 + B_5^2},$$

$$\tan \phi_1 = \frac{B_1}{A_1},\ \tan \phi_3 = \frac{B_3}{A_3},\ \text{and}\ \tan \phi_5 = \frac{B_5}{A_5}.$$

If one term of this electromotive force has the same period as the circuit to which it is applied, the amplitude of this harmonic will be greatly increased in the resulting curve. The natural period of the circuit will depend upon the capacity and self-induction in the circuit, the capacity and self-induction being so chosen that the charge and discharge of the condenser will be oscillatory. The period of this oscillation may be made the same as that of the particular harmonic in the impressed electromotive force.

The form of the electromotive force impressed by a particular dynamo can be determined by obtaining the curve of electromotive force and analyzing it. This analysis gives the constants and the phase relations of the various components of equations (12) and (13), and by it is determined the number of harmonics present in the impressed electromotive force and their respective amplitudes. The principles on which this analysis is based are given in Thompson and Tait's *Natural Philosophy*, Vol. I., p. 54, and in Donkin's *Acoustics*, p. 56, and following.

Briefly the analysis is as follows: Measure a large number of equally spaced ordinates in one complete period of the curve, and then multiply each ordinate by the sine of the angle which corresponds to the position of the ordinate in the curve. Divide the algebraic sum of all the products by the number of ordinates measured, and twice this quotient is A_1, the amplitude of the sine component of the fundamental curve. The ordinates multiplied by the cosines of the corresponding angles, and treated in the same way, give the amplitude of the cosine component of the fundamental. The amplitude of the harmonic components are determined in the same manner by multiplying the ordinates by the corresponding sines and cosines of the harmonic, one degree in the fundamental wave corresponding to three degrees in the third harmonic, and five degrees in the fifth harmonic.

The above analysis applies to periodic curves, but the current is of interest before it reaches its fixed form after the circuit has been closed.

In equation (11) the exponential terms decrease as t increases, and become inappreciable in a fraction of a second; but they modify the character of the curve considerably when the circuit is first closed.

The exponential term may be written[1]

$$i_\iota = A\epsilon^{-\frac{Rt}{2L}} \sin\left\{\frac{\sqrt{4LC - R^2C^2}}{2LC}t + \phi\right\}, \tag{14}$$

where A and ϕ are constants of integration.

These constants are determined by the condition that $i = 0$ and $q = 0$, when the circuit is closed. They are found to be

$$\phi = \cot^{-1} - \left\{\frac{2\cot\psi_1 + RC\omega}{\omega\sqrt{4CL - R^2C^2}}\right\} - \frac{\sqrt{4CL - R^2C^2}}{2CL}t_1, \tag{15}$$

and

$$A = -\frac{2I\epsilon^{+\frac{Rt_1}{2L}}}{\omega\sqrt{4CL - R^2C^2}}\sqrt{(CL\omega^2 - 1)\sin^2\psi_1 + \tfrac{1}{2}RC\omega\sin 2\psi_1 + 1}, \tag{16}$$

in which I equals the coefficient of the sine in (11), ψ equals the angle of which the sine is taken in (11), and ψ_1 equals the value of ψ at time t_1, when the circuit is closed.

Substituting these values of A and ϕ in (14), the general equation (11) may be written

$$i = \Sigma I\sin\psi - \frac{2I\sqrt{(LC\omega^2 - 1)\sin^2\psi_1 + \frac{1}{2}RC\omega\sin 2\psi_1 + 1}}{\omega\sqrt{4LC - R^2C^2}}e^{-\frac{R}{2L}(t - t_1)}$$
$$\sin\left\{\frac{\sqrt{4LC - R^2C^2}}{2LC}(t - t_1) + \cot^{-1} - \left[\frac{2\cot\psi_1 + CR\omega}{\omega\sqrt{4LC - R^2C^2}}\right]\right\}. \tag{17}$$

This equation means that i will be made up of two oscillating curves. The curve represented by the first term is a uniform curve of a permanent type, while the one represented by the second term dies down rapidly after the electromotive force is first introduced, the rate of dying away being determined by the value of $\frac{2L}{R}$ in the exponent of ϵ. The initial value of this logarithmic decrement curve is given by the coefficient of ϵ. This value will depend upon ψ_1.

When $t = t_1$, the first term is $I\sin\psi_1$, and the second term becomes $-I\sin\psi_1$. Hence, upon closing the circuit the current starts from zero, and the direction and magnitude of the first wave depends upon the relative phases and magnitudes of its two components.

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[1] Alternating Currents, Bedell and Crehore, p. 95.

By the *phase* in which a circuit is closed is meant the phase that the current would have at that instant if it *started* as an harmonic current of its permanent type, having the same phase difference with the electromotive force as it finally assumes.

The phase at which the short-lived current begins is determined by its angle of lag relative to the permanent component. The cotangent of this angle is

$$-\left(\frac{2\cot\psi_1 + CR\omega}{\omega\sqrt{4CL - R^2C^2}}\right);$$

the phase ψ of the permanent current in which the circuit is closed, may be found by beginning with the curve after the short-lived component has become inappreciable, and plotting this permanent curve back to the point where the circuit was closed, or what amounts to the same thing, by superposing upon the irregular curve a curve of the permanent type.

Curve No. 12 is the curve of a transformer when fed by a Brush alternator. The curve was obtained with the instrument described in this Journal, Vol. IV, p. 132, and though primarily a current curve, it is similar to the electromotive force curve, since the circuit contained neither self-induction nor capacity. Analysis shows it to be represented by the following equation:

$$e = 1.7026\sin\omega t - 0.00013\cos\omega t - 0.0185\sin 3\omega t - 0.0156\cos 3\omega t.$$

The fundamental makes 117 alternations per second, as shown by the tuning-fork trace on the plate. Hence its third harmonic makes 351 alternations per second.

Curves Nos. 13, 14, and 15 were obtained from the same transformer and alternator as No. 12, but the circuit contained condensers and a self-induction coil in series, the number of condensers in circuit being different for the different curves. The conditions were otherwise unchanged, except that the alternator was not running at quite the same speed in all cases.

Curve No. 16 was obtained from the same transformer and under the same conditions as was No. 12, except the transformer was supplied by a Thomson-Houston alternator, curve No. 17 was from the transformer and Thomson-Houston alternator with condensers and self-induction coil in the circuit, the self-induction and capacity being the same as in No. 13.

Curves No. 18 and 19 were obtained with the same constants in the circuit as was No. 13, but show the effect of the exponential terms in equation (14). The three curves are identical after these terms have become negligible.

Nos. 18 and 19 differ only in that the falling plate happened to close the circuit in different phases.

The phase of closing the circuit in No. 18 was determined by superposition, as indicated above. The phase of the lag of the short-lived curve, the initial value of its logarithmic decrement, the time constant of this decrement curve, and the maximum value, I, of the permanent curve were then computed, and with these values curves were plotted which agree very closely with the curve obtained from the machine.

The accompanying table gives the constants for the different curves.

In this table it is assumed that the self-induction of the transformer is negligible in this circuit, so that the period of the condenser, the time constant of the circuit, and other expressions involving self-induction are not changed by the influence of the transformer.

This assumption is allowable because the alternator and transformer serve to produce a potential difference at the transformer terminals, varying according to a certain law, and this law of variation will not be changed by connecting the terminals through a circuit except in so far as the resulting current reacts upon the transformer. In the present case a large transformer was used, and the current through the secondary circuit was never greater than 0.26 ampere, surely too small to change either the form or the magnitude of the electromotive force. An examination of the table shows that the presence of the condensers emphasizes the third harmonic in every case, but a comparison of curves 12, 13, 14, and 15 is especially interesting, since they are all obtained from the same type of electromotive force.

The values of the constants show that in curve 12, when the condensers are not in circuit, the amplitude of the fundamental is

Constants.	Curve No. 12.	Curve No. 13.	Curve No. 14.	Curve No. 15.	Curve No. 16.	Curve No. 17.	Curve No. 18.	Curve No. 19.
	Transformer supplied by Brush alternator.	Obtained from No. 12.	Obtained from No. 12.	Obtained from No. 12.	Transformer supplied by T. H alternator.	Obtained from No. 16.	Obtained from No. 12.	Obtained from No. 12.
R, ohms	360.000	5.100	5.100	5.100	360.000	5.100	5.100	5.100
L, henrys	0.000	0.036	0.036	0.036	0.000	0.036	0.036	0.036
C, microfarads . .	0.000	8.310	6.780	5.540	0.000	8.310	8.310	8.310
Periodicity $= \frac{1}{T_a}$, of alternator . . .	117.000	116.500	111.000	113.000	125.000	125.000 ?	112.000	112.000
Periodicity $= \frac{1}{T_b}$, of 3d harmonic . .	351.000	349.500	333.000	339.000	375.000	375.000 ?	336.000	336.000
Periodicity $= \frac{1}{T_c}$, of condenser . . .		300.000	328.000	360.000		300.000	300.000	300.000
K_1	1.702	2.880	2.090	1.670	1.703	3.040		
K_3	0.027	0.710	1.860	0.830	0.227	2.080		
K_5					0.215	0.530	Same as No. 13 after they have become permanent.	
ϕ_1	$-0° 0' 18''$	$-2° 10'$	$-19° 55'$	$-36° 30'$	$-2° 10'$	$-10' 25''$		
ϕ_3	$+40° 10'$	$+1° 50'$	$+9° 40'$	$+23° 20'$	$-35° 00'$	$+6° 43'$		
ϕ_5					$+2° 10'$	$-38° 40'$		

sixty-three times that of the third harmonic. In No. 13, though the period of the condensers lacks considerable of being in unison with the third harmonic, yet the amplitude of the fundamental is only *four* times that of the third harmonic. In No. 15, with more nearly unison, the amplitudes are in the ratio of *two* to *one*, and in No. 14, with still better unison, the amplitude of the fundamental is only *one and twelve-hundredths* times that of the harmonic.

Nor is this increase in the amplitude of the *third* harmonic the only change produced by varying the capacity of the circuit. ϕ_3 is given in the table in terms of the angles for the *third* harmonics. Dividing by three to reduce it to degrees of the fundamental, and taking account of the algebraic signs of the sine and the cosine components of the harmonic, the position of the harmonic may be expressed by saying that it is 13° 22′ ahead of opposition in phase to the fundamental in curve No. 12, 3° 25′ ahead in No. 13, 23° 10′ ahead in No. 14, and 44° 20′ ahead in No. 15.

The close agreement of the curves obtained by plotting the fundamentals and *third* harmonics found by the analysis of these curves shows that no higher harmonics are present, but the *fifth* harmonic has almost as large an amplitude in No. 16 as has the third. Note also that the condensers placed in the circuit to obtain No. 17 increased both the fundamental and the fifth harmonic, but in not nearly so large a ratio as the *third* harmonic is increased, and that with the condensers as far out of unison as in any case tried. The increase of the fundamental is due to the fact that the resistance is decreased in greater effective ratio than the self-induction and capacity are increased rather than to any true resonance.

Referring to equations (11) and (17) for curve No. 18, I is computed to be 0.037 C. G. S. units, the initial value of the logarithmic decrement curve $= 0.0578$, $\psi_1 = 61° 24'$, $\chi = -34° 30'$, the time-constant $= 0.014$ sec., and the period of the short-lived current $= 0.0034$ sec. Plotting the first periods with these values gives close agreement with the original curve.

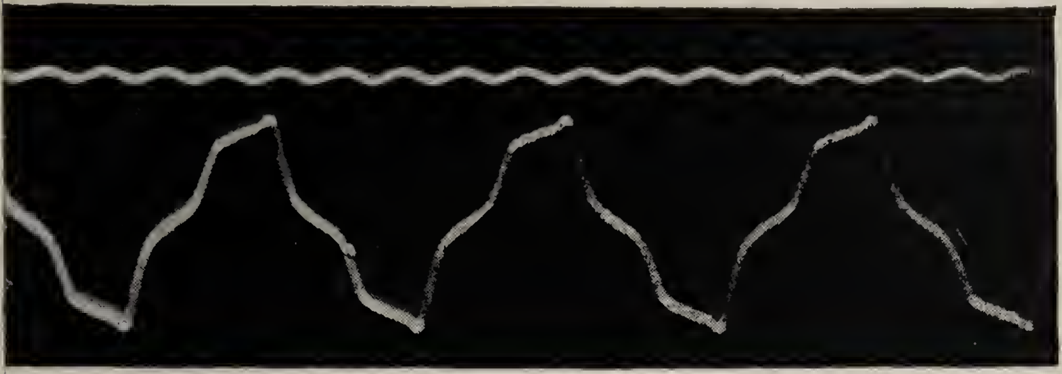
Lantern slides were made from the negatives with the camera. With these slides the curves were thrown on paper screens, enlarged to about six times the size of the cuts here given, and carefully traced with a pencil.

From these tracings the measurements were made. The measurements were expressed in inches, which gave the constants expressing the amplitudes of the components their particular value.

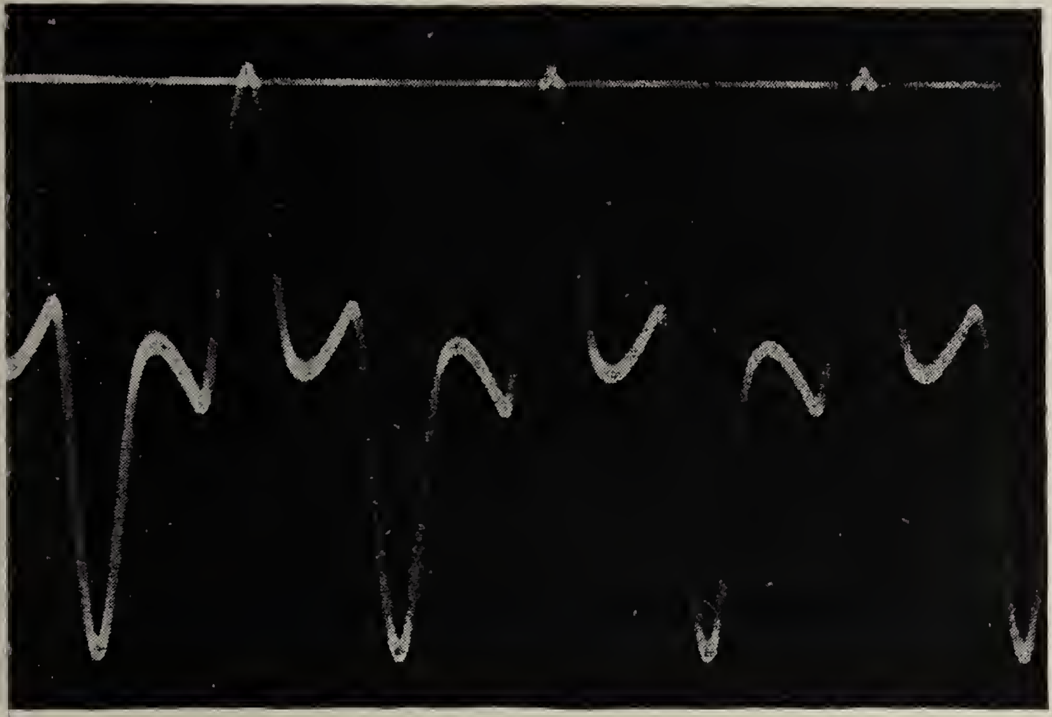
ORDINATES OF CURVES.

	No. 14.		No. 15.		No. 16.	
	Ordinates measured on original curve.	Ordinates measured on platted curve.	Ordinates measured on original curve.	Ordinates measured on platted curve.	Ordinates measured on original curve.	Ordinates measured on platted curve.
1	−1.30	−1.36	−1.40	−1.40	0.075	0.075
2	−1.56	−1.62	−1.38	−1.38	0.125	0.100
3	−1.75	−1.76	−1.29	−1.33	0.200	0.180
4	−1.81	−1.70	−1.16	−1.18	0.280	0.300
5	−1.66	−1.45	−0.96	−0.90	0.420	0.460
6	−1.20	−1.05	−0.73	−0.62	0.610	0.660
7	−0.72	−0.53	−0.34	−0.24	0.870	0.910
8	0.05	0.15	+0.10	+0.15	1.115	1.160
9	0.99	0.86	0.45	0.60	1.380	1.280
10	1.70	1.59	0.82	1.00	1.500	1.490
11	2.34	2.25	1.22	1.38	1.580	1.550
12	3.05	2.95	1.66	1.70	1.635	1.580
13	3.46	3.36	1.96	1.93	1.670	1.600
14	3.68	3.72	2.08	2.06	1.700	1.640
15	3.76	3.80	2.17	2.10	1.730	1.660
16	3.69	3.73	2.15	2.04	1.760	1.710
17	3.34	3.43	2.02	1.92	1.790	1.760
18	2.85	2.95	1.82	1.78	1.850	1.840
19	2.29	2.38	1.69	1.54	1.910	1.880
20	1.81	1.80	1.31	1.34	1.950	1.840
21	1.34	1.26	1.16	1.14	1.840	1.700
22	0.70	0.55	0.95	0.95	1.470	1.500
23	0.30	0.21	0.84	0.83	1.060	1.180
24	−0.02	−0.14	0.77	0.74	0.700	0.900
25	−0.20	−0.25	0.79	0.75	9.495	0.600
26	−0.17	−0.20	0.88	0.84	0.330	0.340
27	0.00	0.00	0.98	0.94	0.225	0.160
28	+0.27	+0.25	1.14	1.07	0.135	0.060
29	0.54	0.60	1.24	1.21	0.073	−0.030
30	1.03	0.95	1.31	1.30	0.000	−0.060

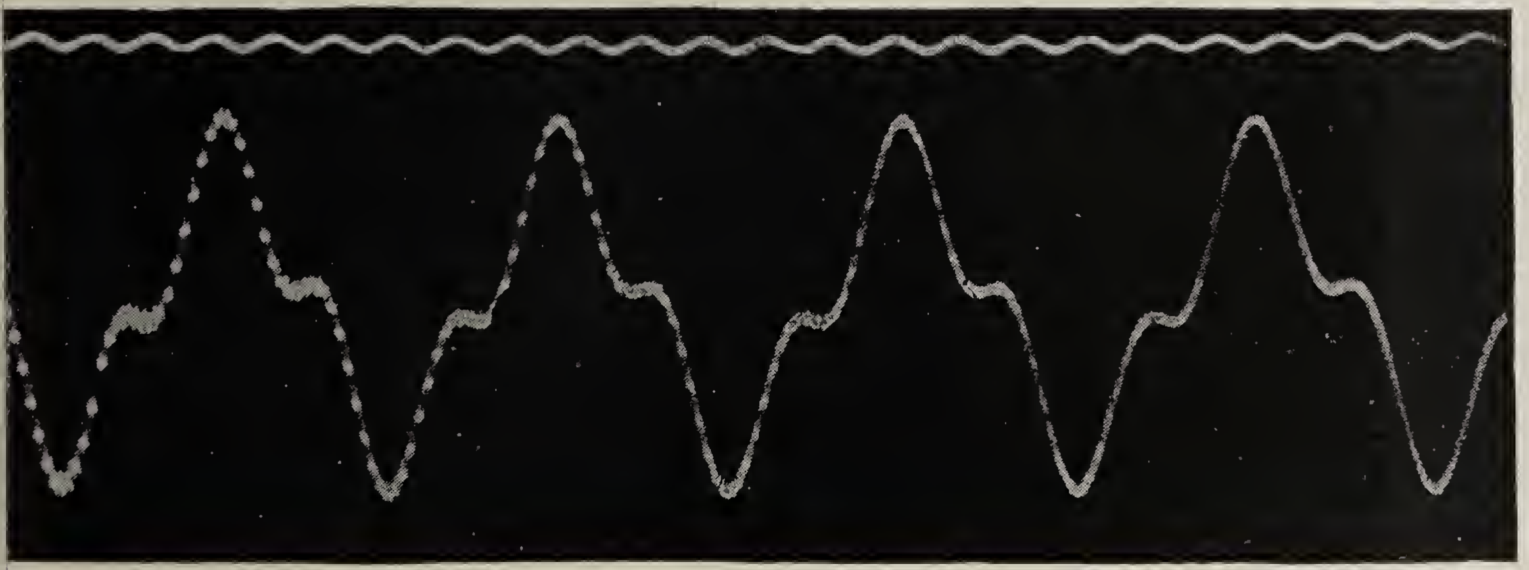
Ordinates were measured every six degrees; that is, sixty ordinates were measured in every complete period. A comparison of the ordinates measured on the curves with corresponding ordinates obtained by plotting the component curves with their phase differences, as computed from the analysis, gives a much better idea of the degree of similarity between the curves than the mere placing of the plotted curve beside the original. That such a comparison may readily be made, a table is given containing side by side the ordinates of one-half period of each of three curves. The greatest discrepancy in any instance is 0.21 inch, and the two curves lie very close together, even for that difference, since at that portion of the curves they are falling rapidly.



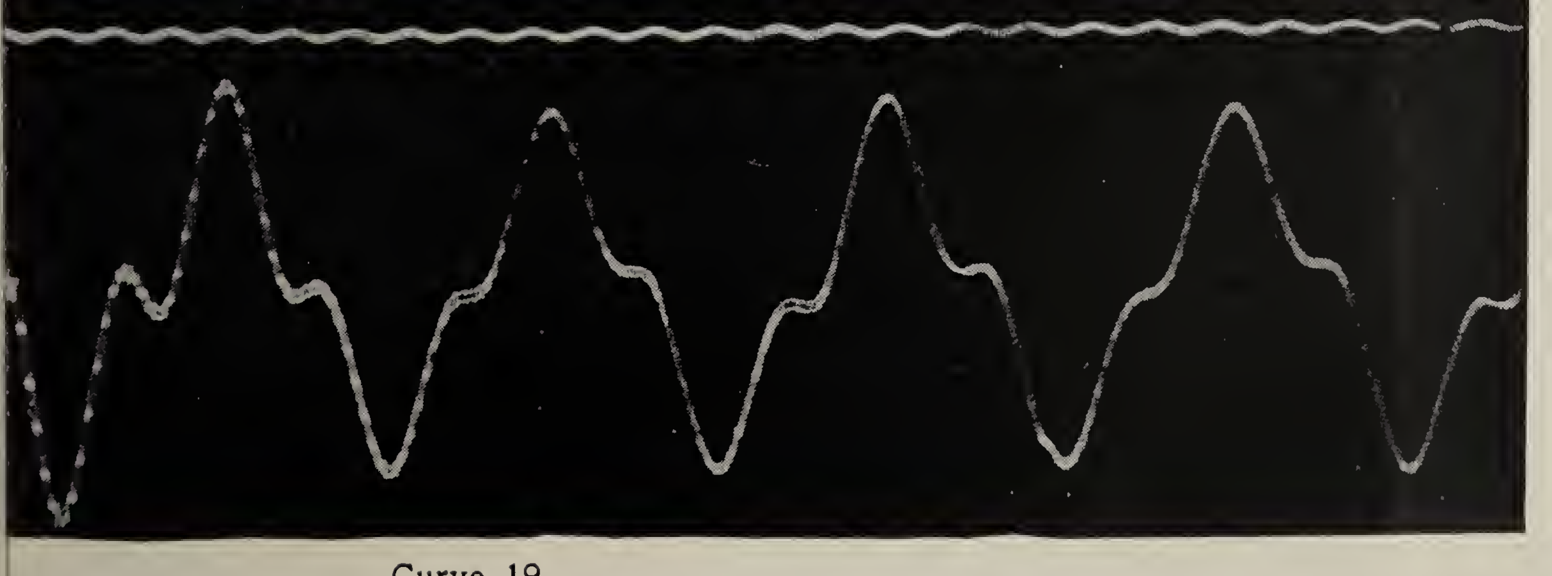
Curve 16.



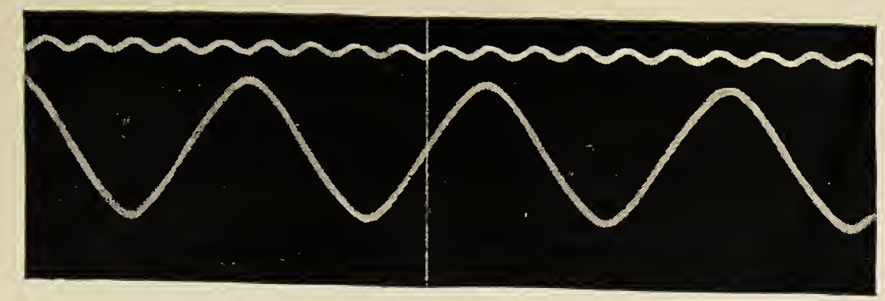
Curve 17.



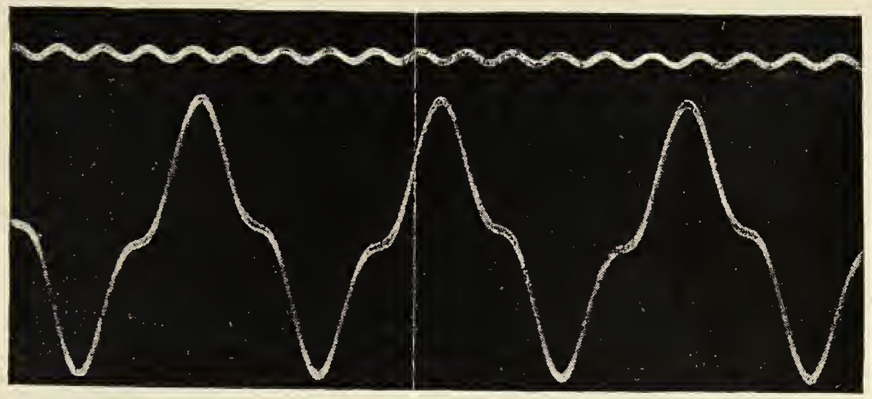
Curve 18.



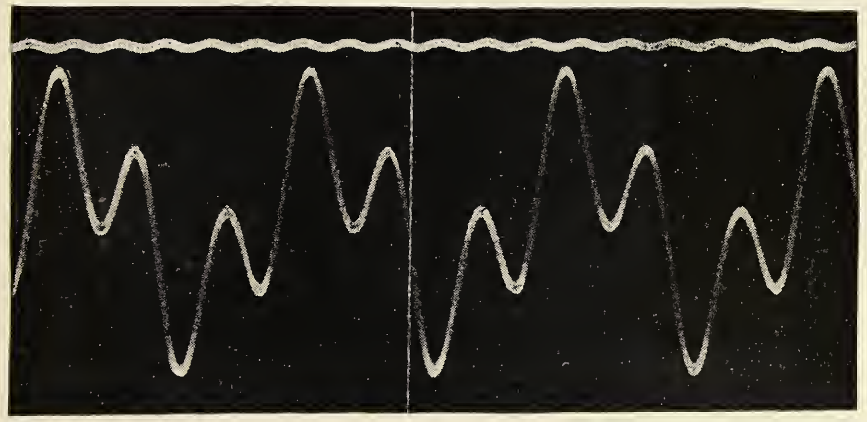
Curve 19.



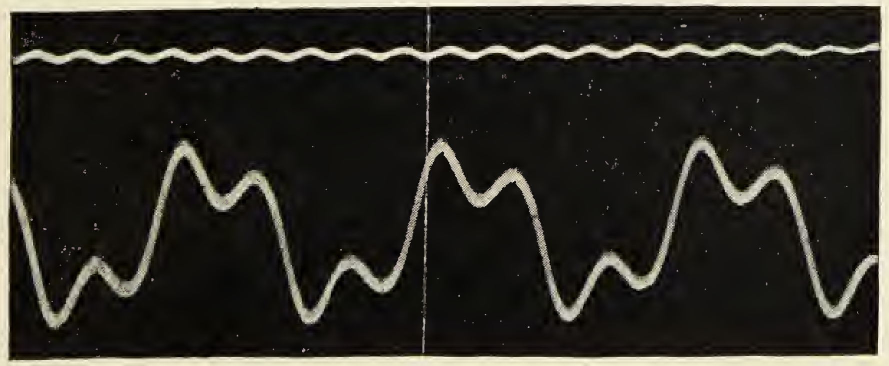
Curve 12.



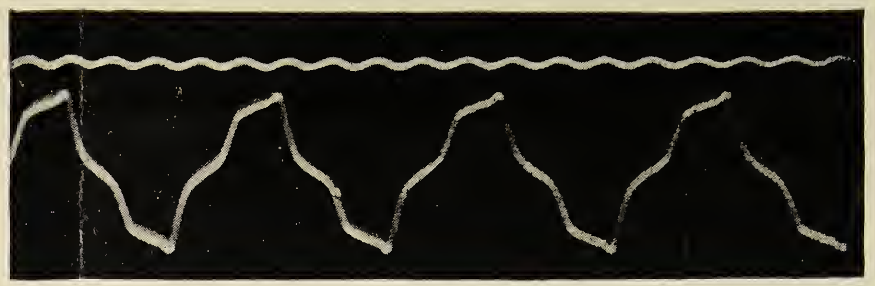
Curve 13.



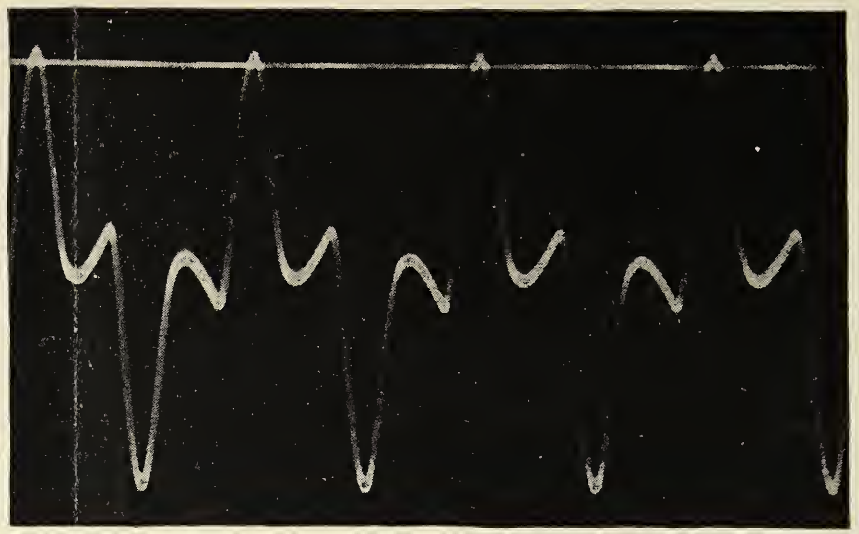
Curve 14.



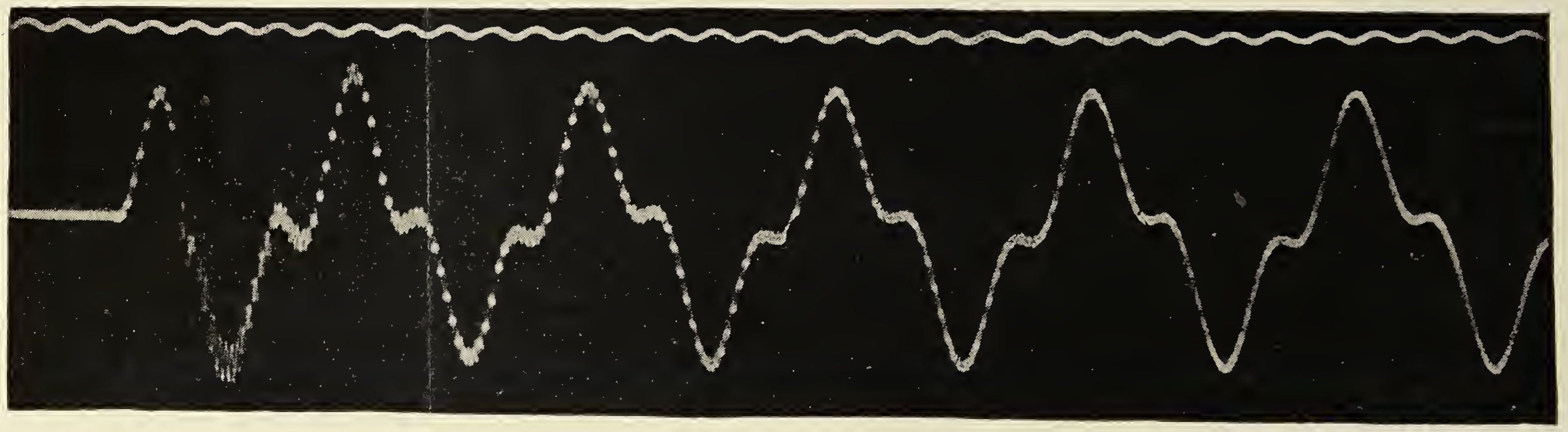
Curve 15.



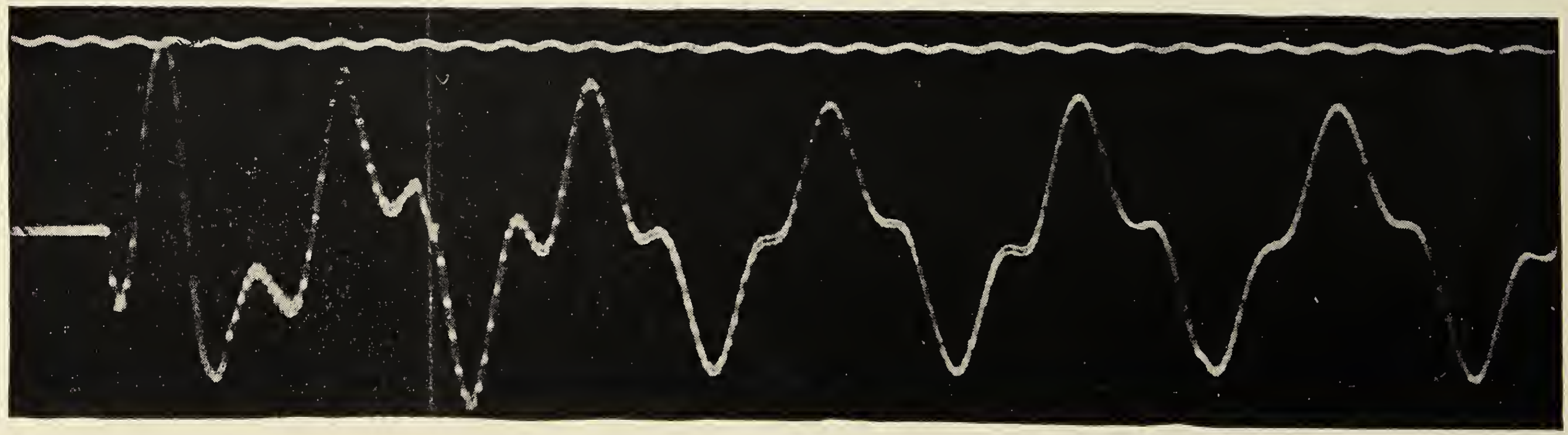
Curve 16.



Curve 17.



Curve 18.



Curve 19.

MILLIS: RESONANCE IN ALTERNATING CURRENT CIRCUITS.

ON THE CONVERSION OF ELECTRIC ENERGY IN DIELECTRICS.[1] II.

By Richard Threlfall.

TWO *ebonite spheres* cut from the same rod of the Silvertown Rubber Company's best ebonite. The smaller sphere was formed from the central part of the rod, while the larger was only slightly less in diameter than the rod itself.

The dimensions are —

Diameter, larger sphere	1.5 cm.
Diameter, smaller sphere	0.713 cm.
Mass, larger sphere	2.097 g.
Mass, smaller sphere	0.214 g.

The resulting curves were the same whatever the surface of the ebonite.

For the larger sphere,

$$W = 0.017\,F^{1.85},$$

and for the smaller,

$$W = 0.03\,F^{1.85}.$$

The index of F was absolutely the same for the two spheres. The great difference in the coefficients, however, excited surprise at the time, as no such inconsistency had been met with up to then, and the experiments were therefore repeated and varied in every possible way. For instance, the same quartz fiber was used for both spheres so as to get different deflections; then separate threads were used so as to make the deflections about equal. The spheres were dried and warmed by flame gases and tested one after the other over the whole range as quickly as possible. In another experiment each sphere was dried for several days, as usual, before measurements were made.

All these trials resulted in the same thing — identity of index, and variability of coefficient of F. The only possible explanations of the difference are, either that an outer shell of an ebonite rod differs from the core, or that the difference of size with respect to the guard tube has a specific effect. The latter hypothesis was tested, as far as possible, by casting spheres of different materials of different sizes, but it soon appeared that the spheres were so variable in property among themselves that no two, even of the same size, could be got to behave alike. Consequently, I cannot prove formally that the observed difference with the ebonite spheres is not due to the action of the guard tube, but, taken in conjunction with the other facts, there is practically no doubt as to the difference being real.

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[1] Continued from the Physical Review, Vol. IV., p. 479.

The relation between speed and deflection was tested with the smaller sphere, with the following results. Potential difference, 40,000 volts.

Frequency of rotation.	Double deflection in centimeters.
9.1	26.45
16.0	26.64
25.0	26.44

Glass.

Only one kind of glass was used, viz. English flint glass made by Messrs. Powell & Co., of Whitefriars. Out of this two spheroids and several cylinders were constructed, and several trials were made with them. Owing to a variety of reasons, observations were practically limited to one spheroid.

Longer axis	2.244 cm.
Shorter axes	0.926 cm.
Density	3.1306
Volume	1.0107 (measured, as the figure was not very perfect).

With regard to the value to be assigned to the specific inductive capacity, the data was not very satisfactory. Hopkinson measured the specific inductive capacity of a sample of Chance's "Light Flint," which has a density of 3.2, and is therefore probably very similar to Powell's glass. The Light Flint gave a value for K which may be taken at 6.7.

This, with the above dimensions, leads to an internal screening factor of 0.233.

A large number of experiments were made with this spheroid. The relation between potential difference and angular deflection was examined in the three tubes — glass, mica, and ebonite — at twelve points between 0 and 6000 volts. The above experiments were repeated in two tubes, the glass being unvarnished, and were also performed both for varnished and unvarnished glass at high and at low frequency of field rotation. It was found that varnishing the glass, or varying the speed of rotation, or varying the guard tube, had absolutely no effect on the index of F, though, of course, the coefficient of F varied, according to the guard tube employed, owing to the screening inconsistencies. These experiments, more than any others, were relied upon in establishing the validity of the experimental method. A great number of measurements were also made with the spheroid and guard tube in different conditions as to surface moisture, and, indeed, it was in this way that the dependence of the results upon the hygrometric state of the air was first discovered. The final result arrived at for varnished glass, when the tube and spheroid were both dry, — temperature 26° C. and frequency of rotation 18.8, — is

$$W = 0.038\,F^{1.92}.$$

When $F = 1$, the loss is about 14 per cent, and the percentage loss for other values of F is

$$\text{Percentage loss} = 14 \times F^{\,0.08}.$$

The curve was not very even, owing to the assistant at the voltmeter not having acquired sufficient skill, and, therefore, a special examination was made to find whether an index of 2 would not conceivably satisfy the results. This proved to be impossible.

For comparison, the following results are stated; they were obtained under varying conditions of dryness of the spheroid and tube, and illustrate the importance of having things dry. The index is correct but the coefficient approximate.

(1) Guard tube dry; glass dried carefully by hot gases, but not left over phosphorus pentoxide:

$$W = 0.026\,F^{1.85}. \quad \text{Frequency, 26.}$$

(2) Ditto. Frequency, 4 per second:

$$W = 0.036\,F^{1.85}.$$

(3) Spheroid dried by P_2O_5; tube damp. Frequency, 6:

$$W = 0.07\,F^{1.72}.$$

(4) Very damp day; spheroid dried by P_2O_5 for 4 days:

$$W = 0.03\,F^{2.2}. \quad \text{Frequency, 25.}$$

On this occasion the tube was perceptibly deficient in insulating power, and this is almost the only experiment which showed a rise of index due to dampness. Since it is probable that the tube became electrified quite unevenly and that the field was far from uniform, no weight is to be attributed to the result, and it is only introduced here to show how great an effect dampness may have on the results. On blowing hot air on the guard tube the deflection at once increased.

Instead of tracing the variation of the coefficients with change of frequency, it will suffice to reproduce two sets of deflections at varying speeds. Of these, one refers to experiments on the spheroid on a dry day — when the tube was dry — and the spheroid dried by hot air before being mounted in the tube, but not by P_2O_5, and the other to the case of both the tube and spheroid being as dry as possible, *i.e.* to the same set of experiments as gave the value of W adopted here.

SPEED AND DEFLECTION.

Spheroid of varnished glass, imperfectly dried; tube dry; potential difference, 5000 *volts.*

Frequency.	Double deflection.	Frequency.	Double deflection.
4.0	14.66	22.4	10.04
11.7	12.18	16.0	9.46
18.0	10.72	27.8	9.10

RELATION BETWEEN SPEED AND DEFLECTION.

Varnished glass, perfectly dry.

Frequency.	Double deflection.	Frequency.	Double deflection.
18.8	31.33	8.0	31.55
26.5	31.54	18.8	31.65
26.5 (another set)	31.58		

It is thus clear that the effect of varying the frequency of cell rotation is almost without influence on very dry glass, but that the effect on imperfectly dried glass is very marked. The variation in the latter case has all the regularity of a definite physical phenomenon, but so far as the properties of glass are concerned, it is wholly without meaning.

Sulphur.

Four samples of sulphur of a high degree of purity were prepared by the method described in a paper on the "Electric Properties of Pure Sulphur" (*Phil. Trans.*, as yet unpublished). Briefly, the method consists in filtering the very pure sulphur obtained by the Chance-Claus process; distilling and finally exhausting *in vacuo.* (In the present case the exhaustion *in vacuo* was omitted.)

Spheroid A. — Perfectly annealed sulphur, of the variety which I have ventured to describe (*loc. cit.*) as "aged monoclinic sulphur," *i.e.* sulphur which preserves the melting point and physical properties of monoclinic sulphur, but which has lost the crystallographic properties of that variety.

Longer diameter	2.664 cm.
Shorter diameters	0.976 cm.
Weight	2.5625 g.
Volume	1.2945 c.c.
Density	1.9.

The proper density is 1.98, so that there was probably a small cavity in the spheroid.

Specific inductive capacity . . . . 3.162 (*loc. cit.*).

These data give a screening factor of about 0.44. This spheroid was tested by complete sets of observations in glass, mica, and ebonite guard tubes, at high and low frequencies of rotation. The weather was dry, and everything insulating well, including the spheroid, which was specially tested, but no drying material was employed.

It was found that the apparent loss, under these circumstances, depended very much on the speed of cell rotation. Thus, at a frequency of 21.9, the deflection with a potential difference of 5000 volts was 151′, and with a frequency of 4, under similar conditions, it rose to 270′. Complete sets of observations of variation of deflection with speed were made both in glass and in ebonite, but in view of what has been already said on this head, there is no point in reproducing them.

The final result, at a frequency of 22 and a temperature of 25.2° C., was

$$W = 0.0139\, F^{1.91},$$

$$\text{Percentage loss} = 11.05\, F^{-0.09}.$$

An index of 2 would not satisfy the observations, which were very regular. The sulphur was lightly paraffined during the tests from which the above result is deduced; but it was found, by comparing these with other tests of observations made before the paraffining was done, that the paraffin had no effect.

Spheroid C. — Similar in all respects to spheroid A, except that it was perfectly dried. This had the effect of reducing the deflection so much that it could not be measured with sufficient exactness to construct a curve of any pretensions to accuracy. For instance, with a potential difference of 4000 volts, the double deflections in centimeters were spheroid A, 11, and spheroid C, 2.2. The results, such as they were, pointed to an index of F of about 1.5.

After some ineffectual attempts to measure the loss with other ellipsoids, a complete series of experiments was made with —

Spheroid K. — Although it had been annealed, an analysis of a test piece showed that it contained about 7 per cent of insoluble sulphur. As this spheroid has been mentioned in the earlier part of the paper, I will merely repeat that the result at about 18 rotations per second was

$$W = 0.00048\, F^{1.857} \text{ at } 17.8^\circ \text{ C.}$$

The specific inductive capacity being somewhat low (viz., say 3.2), this leads to a very small percentage loss, viz. 0.0038 per cent at $F=1$; and the law of percentage loss is

$$\text{Percentage loss} = 0.00377\, F^{-0.143}.$$

As the other samples of sulphur appeared to show a very considerable variation of loss as the frequency varied, this was now made the subject of an elaborate inquiry. It was found, after a good many tests made in air of varied humidity, that the phenomenon of variation of loss with variation of frequency was a property of the sulphur and not of the apparatus; but it was much smaller than when the sulphur had not been specially dried.

VARIATION OF DEFLECTION WITH FREQUENCY OF CELL ROTATION.

Potential difference, 3500.

Frequency of cell rotation.	Double deflection in centimeters at 96 cm. scale distance.
30	16.47
18	14.00

This result differs from that obtained for spheroid A, in that the loss appears to increase as the speed increases, but spheroid A might have been slightly affected by dampness. Space will not permit of the detailed description of the series of experiments leading to the above statement for spheroid K, but it is necessary to say that the matter was thoroughly tested on many occasions, and always with the above result. One very good way of making a test is to alter the speed of rotation while the voltage is maintained constant. As this could be done by myself from my seat at the dynamometer scale, I was able to increase or decrease the deflection at will, and thus assure myself of the fact. The above numerical estimate is, of course, based on sets of observations made with the cell rotating in both directions.

Paraffin.

Spheroid A. — Made out of paraffin melting at about 50° C. This spheroid was tested by a thread which had been used satisfactorily for an ebonite spheroid of the same size. The resulting deflections were so small that no certain conclusion could be drawn from them.

Spheroid B. — Made out of paraffin of melting point 52° (Field's hardest) by turning.

Longer diameter	2.64 cm.
Shorter diameters	0.914 cm.
Volume calculated	1.170 c.c.
Specific inductive capacity assumed	2.3
Screening factor	0.3556

On testing this without phosphorus pentoxide a tolerable curve was obtained with an index of F of 1.38. The effect did not depend on the speed of rotation of the cell. After prolonged drying the spheroid was examined by means of a very fine fiber having a coefficient of torsion only one sixteenth of that of the fiber used for ebonite, glass, etc. The observations were not very satisfactory owing to unsteadiness of zero; however, they plotted out to a very straight line, making the index of $F = 1.95$, at a temperature of 13°.3 C., instead of 1.38 which was got when the spheroid was not artificially dried. The loss of energy was very small indeed.

It was obviously necessary to make fresh observations on a larger spheroid with smaller internal screening; consequently —

Spheroid C was tested. This was cast in a zinc mold from the same paraffin as spheroid B. The paraffin had been kept melted for some time in great excess, and the spheroid was cast by decanting off the upper layer of the melted paraffin.

Dimensions:

Longer diameter (and axis of suspension)	1.80 cm.
Shorter diameters	1.38 cm.
Volume	1.802 c.c.
Internal screening factor	0.658

Observations were made at a temperature of 12°.5 C. — two complete sets at frequencies of 12.5 and 27.3, besides two subsidiary sets on days when the air was in a different hygrometric state, for purposes of confirmation. The observations were satisfactory in spite of great zero changes at both speeds; and the effect was reduced by raising the frequency. Thus, with a frequency of 12.5 and a potential difference of 6000, the double deflection was 3.28; and with a frequency of 27.3 and a potential difference of 6000, the double deflection was 2.92. This was confirmed by direct observation, viz. by changing the speed of cell rotation and noting the effect on the light spot. The logarithmic curves at the two speeds were exactly parallel, and gave 1.56 as the index of F.

The final result was:

At a frequency of 12.5 and temperature 12°.5 C.,

$$W = 0.00138\, F^{1.56}.$$

At a frequency of 27.3 and temperature 12°.5 C.,

$$W = 0.0008\, F^{1.56}.$$

The percentage losses at $F = 1$ are:

At a frequency of 12.5, percentage loss per cycle $= 1.51\, F^{-0.44}$.
At a frequency of 27.3, percentage loss per cycle $= 0.88\, F^{-0.44}$.

Various spheres of paraffin were also tested in making a comparison between the effect with pure paraffin and the effect with paraffin made exceedingly heterogeneous, but still insulating as a whole, by means of powdered Ceylon graphite. This will be dealt with later on; it is only necessary to mention here that the index of F obtained in these experiments was from about 1.5 to 1.6. The spheres being hung in the deflection tube while still warm, and the tube itself having been dried for a long period by plenty of phosphorus pentoxide, it was again noted that shifting of the zero was exceedingly marked with paraffin.

Selenium.

I have in my possession a large quantity of selenium, which I owe to the liberality of the Royal Society of London. During the last two years I have devoted a considerable time to the purification of this substance; the selenium as purchased containing about one half a per cent of impurity, chiefly glass, iron, sand, and copper. I have succeeded in effecting the purification of about one kilogram of this material, but am not quite satisfied as to the absence of oxides of selenium and of very minute traces of arsenic. I was enabled, therefore (within these limits), to examine the properties of selenium fairly closely. I have not, however, as yet been able to make a satisfactory measurement of the specific inductive capacity of the substance in its various modifications, for the conduction data are yet to be obtained. A determination of the specific inductive capacity of selenium in the vitreous — or fairly non-conducting state — by Kelvin's method (comparison of the condenser capacities by the galvanometer) gave a value of about 30 for the specific inductive capacity. The conductivity, however, is too great for this method to be really satisfactory, even when all precautions are taken. I therefore state the result with every possible reservation, and without laying any stress upon it. Some value or other is requisite to enable the energy loss to be calculated, and the experiments on this head were highly satisfactory throughout. When a final value for K is obtained, the energy loss formula can be recalculated without this part of the work requiring revision.

Selenium Ellipsoid A. — Cast from the selenium as purchased. The deflection in the rotating field was found to be of a quite new order of magnitude, and was, in fact, so large that experiments with this ellipsoid were abandoned on account of its size — it was evidently necessary to be sure the field was uniform.

Sphere B. — Diameter 0.7 cm. Weight 0.701 g. (which shows that it was probably not solid). The density of selenium in the vitreous state is said to be 4.25, while the calculated density of this sphere was only 3.9.

This sphere was made by heating the selenium (commercial) to the boiling point and then pouring it into a cold zinc mold, which was then thrown into water. The sphere was not quite non-conducting, *i.e.* it discharged a gold-leaf electroscope perceptibly in 30 seconds when held against the plate. The sphere was tested after diselectrifying and placing, while warm, in the dry tube, and also after two days' drying, when the results were practically the same. The index of the potential difference was 1.96, and the observations were so good that an index of 2 clearly would not satisfy the numbers. This was at a temperature of 14° C. The results are not reduced in the usual manner because the selenium was not pure, and because they are very similar to the next series, except that I could not detect with sphere B any change in the deflection when the frequency varied from 6 to 25.

SELENIUM SPHERE B.

Frequency of rotation.	Single deflection in minutes.	Potential difference in volts.
10	693	5000
18	709	5000
25	692	5000

Selenium Sphere C. — Commercial selenium. Diameter, 0.7 cm.; weight, 0.8855 g.; apparently perfectly insulating, as tested on the electroscope. This sphere was evidently a solid sphere without bubbles, the calculated density being 4.93, while the density of vitreous selenium is given in the books at 4.28. The sphere was cooled by the mold without immersing in water, and hung by the same thread, under the same conditions as sphere B. It was tested both at once after mounting and after sixteen hours' drying by P_2O_5, which had the effect of *increasing* the deflection, showing that the tube was not quite dry initially. Both curves were practically parallel (index of first set 1.92, and of second set 1.90).

The effect on C was markedly less than on B, which conducted slightly. Thus, at 5000 volts potential difference, the deflection of

B was 709′ of arc,
and of C was 262′ of arc.

Correcting these numbers on account of the evident unsoundness of B, we get

Corrected deflection of B	894′
Corrected deflection of C	262′

or the loss with the uninsulating sphere B is 3.4 times as great as with the insulating sphere C.

The curves were perfectly regular in both cases, and there was no trouble from shifting of the zero. The general formula is not worth obtaining for a sphere of such impure material, but there was no doubt in this case as to the variability of the loss with varying frequency of cell rotation.

SELENIUM SPHERE C.

Frequency.	Deflection in Minutes.	Potential difference in volts.
10	361	5000
18	374	5000
25	377	5000

Selenium Sphere D. — Purified selenium. Diameter, 0.7 cm.; weight, 0.901 g.; cooled by mold only; cast from selenium at boiling point; density calculated from mass and volume, 5.02. This must mean either that the density of pure selenium is incorrectly given, or that the sphere was out of shape owing to the two halves of the mold not being sufficiently closely approximated. I did not care to risk the possibility of inducing a surface change by weighing the selenium in water.

The selenium was tested on one occasion when it was compared with sphere C; both were dried and put into a dry tube, — the air was not very dry (humidity 64 per cent), — but no effect was produced by blowing in hot air. The resulting curve was very good with an index 1.85; the insulation to electroscope test appeared practically perfect. Temperature of test, 15°.3 C. The amplitude was much the same as for sphere C. Thus the observations for both C and D at 5000 volts lie exactly on their respective curves; and both spheres were hung from same thread. The single deflections at this point are

$$\text{Sphere D} = 427, \text{ and sphere C} = 461,$$

showing that the pure selenium suffers a slightly smaller loss.

Taking $\mu = 28$ (for convenience of calculation), the result for sphere D is

$$W = 4.6\, F^{1.85};$$

energy of electrification in one direction is 1.075 ergs ($F = 1$).

Hence the loss appears to be over 400 per cent. I am not prepared to say at this stage whether such an extraordinary result can possibly be due to any error in estimating K, but I think not. The dynamometric results are above suspicion. There is, so far as I can see, no impossibility in the loss being greater than the energy of electrification in one direction; for suppose that polarization is set up and vanishes after a time, short, but not very short compared with one revolution of the field, then the source is drawn upon for a fresh supply of energy and the process is repeated as often as we like. While the charges are distributing themselves by conduction, the angle between the line passing through the center of the sphere and the center of gravity of the charge and the direction of the field, may increase and give rise to the observed increased torque.

On the other hand, if the polarization vanishes in a time indefinitely short compared with the period of field rotation, the angle is practically always zero, so that in perfectly or even in well conducting bodies no dynamometric effect is to be expected. This, of course, is not really the physical case, for if we have a real conductor, *i.e.* one in which conduction occurs in times very short compared with the period of field rotation, then we may get perfect internal screening, and no work of polarization is done, because there is no force except within an infinitesimal distance of the boundary.

Comparing spheres C and D at 5000 volts potential difference, and correcting for the difference in weight (on hypothesis that volumes are proportional to weights), we find —

Loss in sphere D proportional to 427,
Loss in sphere C proportional to 468.

No observations on the variation of loss with varying frequency were made with this sphere.

Resin.

The resin employed consisted of picked pieces of the resin sold as a flux for soldering lead. Experiments were made on spheres of 0.7 and of 1 cm. in diameter, which were cast in zinc molds. A large number of spheres were examined under very diverse circumstances, because there was an utter want of regularity in the phenomena observed. The following comparison will give an idea of the magnitude of the effects.

Small ebonite sphere, diameter say 0.7 cm. nearly:

Deflection in minutes at 5000 volts 400

Resin sphere θ, diameter say 0.7 cm.:

Deflection in minutes at 5000 volts . . . 123

Resin sphere γ, diameter say 0.7 cm.:

Deflection in minutes at 5000 volts . . . 484

The coefficient of torsion of the threads being sufficiently nearly the same.

As the resin has about the same dielectric constant as ebonite, the percentage loss in the case of sphere γ is about 18 per cent of the initial energy of electrification, when F is about 13, or the potential difference of the plates is 5000 volts.

In order to decide whether any explanation of the extraordinary irregularity of the results with resin could be accounted for by any change in the apparatus, the smaller ebonite sphere was remounted and rapidly examined. It was found to give exactly the same results as before, and the zero was perfectly steady. With the resin, on the other hand, the zero was always very unsteady, and great care was requisite to get any reliable results at all. All the spheres were cast from the same sample of resin, and on one occasion two were cast at the same time from the same lot of resin melted in a dish. These two were widely different in their properties. It may be added that owing to the practice of casting the resin in a nearly cold mold so as to avoid the sticking of the resin to the sides of the mold, the resin spheres were always in a state of strain, and generally cracked considerably within twenty-four hours after casting.

The following table will show the uselessness of going into the question of dielectric losses in resin in any detail; it represents but one set of experiments out of many made on this subject. All the experiments here included were made on one day, and under exactly similar conditions; this, of course, means that in this case there is no prolonged drying. The resulting curves (as might be expected from the difficulty of observing) were very irregular as compared with those obtained from other substances, and, therefore, too much stress must not be laid on the value assigned to the index. A typical set of numbers will be given for one sphere, from which the degree of certitude of the result may be, to some extent, inferred. It must not be forgotten, however, that when the zero is subject to a considerable drift, the method of observing by reversing the direction of rotation will not eliminate the drifting, unless the observations are made under comparable conditions as to the time elapsing between the raising of the potential difference to the required point and the taking of the reading. This is not quite easy to do, because the observations of any potential difference consist of the reading of ten elongations (five on each side of the resting point), and difficulties in the way of keeping the potential difference steady often cause a small difference in the time required for the observations.

The effect of increasing the speed of rotation with resin was to increase the deflection: at the same time, the effect was not large. Thus, in the case of a sphere freshly cast for the purpose of a test, the deflections increased from 20.3 to 21.8 scale divisions as the frequency increased from 10 to 25. The creep of the zero was, however, so marked that it is quite possible to account for the variation observed by supposing that the error introduced into the result and remaining uneliminated on reversal of the cell, was the quantity affected by the change of frequency.

RESIN SPHERES.

Deflections and potential difference.

Sphere.	Diameter of sphere.	Rotations in minutes of arc with a potential difference of 5000 volts.	Index of *F*.	Remarks.
β	1.0 cm.	236	2.4	Dried two days.
γ	0.70	489	2.8	Dry in dry tube; taken at once.
δ	1.0	172	2.2	Resin re-heated; observed at once.
ζ	1.0	440	1.86	Same melt as last; observed at once.
θ	0.70	123	2.23	Same melt as last; observed at once.
χ (Resin and graphite)	0.70	442	2.02	Same melt as last; 10 per cent by volume of fine graphite added.

TABLE SHOWING RELATION OF POTENTIAL DIFFERENCE AND DEFLECTION.

Resin sphere γ, 0.7 *cm. diameter.*

Potential difference volts.	Single rotations of the sphere in minutes of arc.	
2000	39	
3000	122	
4000	248	In drawing the logarithmic "plot," weight is given to these observations above that assigned to the other two.
5000	484	

A good many other observations which were made on resin spheres resembled those quoted in giving an index for *F* from about 2.2 to 2.3, but as threads of varying stiffness were used, and as the general results exactly resembled those quoted, there is no reason for reproducing them. I am unable to see any theoretical objection to the index being higher than 2, for if the effect depends in some measure on internal conduction, we know that this factor will itself depend on the potential difference, since substances like resin are far from obeying Ohm's law. As in the case of selenium, which also gave very anomalous results, the resin appeared to insulate perfectly when tested by the gold-leaf electroscope.

DIELECTRIC LOSS IN HETEROGENEOUS SUBSTANCES.

These experiments will be described in the order in which they were made. The intention was to examine the properties of some homogeneous substances, and to compare these properties with those exhibited by the same material when it was purposely rendered heterogeneous by mixing it with some inert but conducting substance. Resin spheres could be cast so as to be perfectly transparent, and it was considered that this transparency would be a guarantee of, at all events, the first order of homogeneity. No doubt the strains set up on cooling introduce a heterogeneity of a certain kind, but it must, from the electrical point of view, be of a single kind only.

The complete and uniform transparency of the substance negatives the possibility of any sudden or appreciable change of the electric or magnetic constants, unless it be assumed that these quantities vary inversely — a theory very far removed from any known facts. The chemical composition must be uniform, because the melted resin is well stirred and cools as soon as it is poured into the mold; the viscosity also is very great. These facts negative any hypothesis of gravimetric separation. The resin does not crystallize under any circumstances, so far as I am aware, and consequently the only form of heterogeneity that can be invoked is such as results from the unequal distribution of strain. It is just possible that the specific resistance may be a function of the strains, and if so, the necessary heterogeneity would be introduced. I know of no facts in support of this theory, except, perhaps, my own observations on sulphur, which showed that the specific resistance is an exceedingly unstable property, and one which is specially sensitive to minute changes of chemical composition. On the other hand, I found that crystalline sulphur may pass from the monoclinic state into a second state, in which the crystallographic properties are entirely lost, without the resistance being in any way affected, *i.e.* the specific resistance under specified conditions as to voltage, etc., may remain above 10^{28} — my limit of discrimination.

On the whole, therefore, it appears —

That the heterogeneity which is required by Maxwell's theory to account for the observed dissipation of energy in clear resin spheres is, *so far as is known*, entirely absent.

In order to test the Maxwell theory, therefore, it appeared to be necessary to introduce, purposely, a degree of heterogeneity indefinitely great as compared with anything naturally existing in clear resin. The kind of heterogeneity required is such as to make the product specific resistance × specific inductive capacity vary from point to point. Now, as I have said, the "specific resistance" of such a substance as resin does not exist for the substance in bulk, but it may be supposed that it exists for exceedingly small portions of the substance, *i.e.* though a plate of resin does not obey Ohm's law, a particle may do so. This is the assumption made above. It is by no means easy to arrange an experiment in which we can be sure that the heterogeneity is greater than that due to the pure substance, say, resin, for we have practically only two courses open to us: we can introduce a conductor and distribute it through the mass, or we can introduce an insulator with a value of K differing from that of the substance. In the former case, we are in the dark as to the specific inductive capacity of the substance added. For instance, I added graphite, which conducts, say, 10^{14} times better than resin for electric intensities such as were employed. This estimate, of course, is very rough. Is it possible that the specific inductive capacity of graphite can be greater than that of resin in this ratio? On the other hand, if an insulator, such as powdered flint glass, be added, we have, say, a threefold change of specific inductive capacity, and, perhaps, a corresponding change in resistance; for it may be urged that we cannot argue as to the resistance of small particles from the resistance observed in plates; especially when, as in the present case, the actual resistances are practically beyond our powers of discrimination. Unless there is some physical law connecting the relation of specific inductive capacity with resistance, the probabilities are all in favor of a heterogeneity of the kind aimed at being introduced by the addition of graphite to resin.

As was stated at the commencement of the paper, therefore we cannot hope to settle the question of the theory of heterogeneity however we try to vary the constitution of the substance examined; we can only hope to raise a probability one way or the other.

My first experiments were made on mixtures of resin and graphite, the latter obtained by grinding Ceylon graphite, of a kind that conducted very well, and passing it through a sieve having 100 threads to the inch. My first experiment was made with a sphere which contained so much graphite that it conducted as a whole, and with it no deflection was produced by rotating the field. The same result was obtained with aluminium, carbon, and tinfoil-coated paraffin ellipsoids. On reducing the quantity of resin to say 20 per cent by volume (it was not measured), I obtained a perfectly black sphere, which when broken had a dull fracture, but which did not discharge an electroscope much faster than a pure resin sphere.

I compared the graphite-resin and the resin sphere, which were made from the same melt of resin, were of the same size, and hung by the same thread. In this comparison the indexes appeared about the same, viz. 1.9, though the results with the resin were very irregular, but the effect was greater in the graphite sphere than in the sphere made of resin only. At a potential difference of 6000 volts, the deflection of the graphite sphere was 128′ and of the resin sphere 25′—an increase in the ratio of 5 to 1.

It was then found that resin is itself exceedingly variable, as shown under that head.

The variability of resin having been ascertained, a fresh trial was made by adding 10 per cent by volume of fine graphite to the resin. The result is indicated in the table of results with resin spheres. The effect of the addition of graphite here appears as practically nil. The curve of potential difference and deflection was quite regular, and the observations at least as satisfactory as with the pure resin.

As M. Hess has given reasons (*loc. cit.*) for thinking that the dielectric loss should be a function of the speed, the variation of deflection with varying rapidity of cell rotation was very carefully studied, with the following result:

GRAPHITE-RESIN χ.

Table showing variation of deflection with varying frequency.

Frequency.	Double deflection in scale centimeters.
10	40.0
18	47.5
28	48.5

This result was obtained in two repetitions of the experiment, and is not to be explained by any assumed moisture effect.

As an additional precaution, the ebonite sphere, formerly studied under very various circumstances, was remounted in place of the resin-graphite, and gave identically the same results as before.

In order to observe whether the deflection speed relation depends on the resin or on the graphite, a new resin sphere was mounted and examined. This sphere was cast from the same sample of resin as the resin-graphite χ. The results were as follows:

Frequency.	Double deflection.
10	40.6
25	43.6

It was clearly desirable to make use of some material giving more uniform results than resin, if such could be found, and, more important still, less affected by the tendency of the zero to shift during observation. To these must be added the qualifications already discussed, as to homogeneity, etc. Practically the only available substance appeared to be paraffin; and even this cannot be prevented from crystallizing to some extent. However, by pouring the paraffin, heated only just above the melting point, into a very cold mold, it was found possible to obtain fairly transparent spheres. On the other hand, paraffin was the substance which, next to resin, had given the greatest amount of trouble by shifting of zero. This peculiarity was found to be strongly developed in the spheres now examined; consequently, there is little more to be learned by the use of paraffin than by the use of resin, and a summary of observations on two spheres will sufficiently meet the case.

Comparison of a sphere of pure paraffin and a sphere of paraffin containing so much graphite that it was just not sensibly conducting:

Temperature, 18°.4 C.; speed, 16 revolutions per second; humidity, 52 per cent; tube, etc., perfectly dry.

Index of *F*, graphite sphere = 2.
Index of *F*, pure paraffin sphere, 1.6 to 1.9. Observations unsatisfactory, owing to zero changes.

Deflections, in minutes, of the spheres at 3000 volts potential difference:

Paraffin sphere,	162	same thread used in both cases.
Paraffin and graphite sphere,	290	

The paraffin-graphite sphere, therefore, experiences an energy loss 1.8 times as great as the loss in the paraffin sphere.

On varying the speed of rotation, the loss with graphite-paraffin was found to increase considerably, as follows:

Speed of rotation.	**Double deflection in scale divisions.**
10	29.2
16	30.0
29	34.8

The pure paraffin sphere gave a similar but rather smaller effect.

Effect on the index of *F* of the addition of graphite to resin and paraffin:

Resin-graphite α	Index = 2.30	Perhaps affected by a damp tube; not much weight.
Resin graphite β	Index = 1.89	Quite dry; nothing known against it.
Resin-graphite χ	Index = 2.02	Quite dry; nothing known against it.
Paraffin-graphite A	Index = 2.00	Quite dry; nothing known against it.

The index thus shows a tendency towards the value 2.

In all cases the effect of a change of speed on heterogeneous material was to give higher values of the deflection with increasing speeds.

The general result may be stated thus: The addition of graphite, either to resin or paraffin, tends to increase the loss and to bring the index towards 2. In the case of resin the change of index is not well marked because the index for resin alone is quite erratic and is already in the mean not far from 2. It is impossible to attempt to calculate the law of loss satisfactorily on the Maxwell theory, as has been done by Hess (*L'Éclairage Électrique*, Vol. 7, 450), because Ohm's law, or some analogous assumption, has to be made and applied to dielectrics. It is a mere case of arguing in a circle to say that "were it not for electric absorption these substances would appear to obey Ohm's law — as the particles in reality do." On the other hand, non-conducting liquids, as examined by Thomson and Newall (*Phil. Proc.*, 1886), appeared to obey Ohm's law, but the elaborate investigations of Köller lead to a totally opposite result. Samples of sulphur containing both soluble and insoluble sulphur examined by me gave exceedingly small residual charge effects, and yet conducted appreciably. They were, however, very far indeed from obeying Ohm's law; in fact, the conduction was not even continuous. A great deal is known about the conduction of very impure selenium, and this substance is not even on speaking terms with Ohm's law, as the researches of Werner Siemens and others have shown. The evidence, therefore, so far as is known, is that substances such as I have examined do not obey Ohm's law in bulk, and till further information is to hand it must be assumed that they do not do so either when reduced to elements of volume. Consequently we cannot hope to test the Maxwell theory by a study of the index of F in the energy-loss equation.

Now as to the magnitude of the increase of loss produced by adding graphite. This is quite inconsiderable in comparison with the enormous increase of heterogeneity which must in all probability (neglecting wild hypotheses) have been introduced. If the original loss, say in paraffin, were produced by heterogeneity, one would certainly have expected that the loss would have been much more than doubled when graphite was added to the verge of conductivity, but, as a matter of fact, the loss was not doubled.

No one doubts, however, that heterogeneity can give rise to a loss of energy, and the question may be put whether the observations recorded above are not such as may be most naturally explained by admitting the possibility of dielectric loss as due to some kind of hysteresis, and supposing that the additional loss introduced when the material is made heterogeneous is a loss to be accounted for on Maxwell's theory, which simply adds on to the normal hysteresis loss. But electric hysteresis, in the sense in which I use the word, may be either static or viscous. The experiments described, however, show that the viscosity, if it exists, is of such a nature as to produce a hardly appreciable effect when the frequency varies from 5 to 30 per second, but, as will be shown presently, is sufficient to obliterate the phenomenon entirely at a frequency of several millions per second.

Experiments on Liquids.

Thomson and Newall (*Phil. Proc.*, 1886) thought they detected a trace of residual charge effect in bisulphide of carbon (*loc. cit.*), and I therefore considered it worth while to see if I could set up a rotation in this liquid. For this purpose a thin flint glass tube was mounted on the stand and dipped into the rotating field in exactly the same position as was habitually occupied by the guard tube. I tried a good many ways of indicating rotation: the most satisfactory of these was to rotate the field, then draw up the tube, and throw some very fine alumina into the liquid. Owing to the low rate at which this substance subsided, its motion could be accurately observed. The liquids examined were carbon bisulphide, benzene (C_6H_6), kerosene (from a fresh tin opened for this purpose), and distilled water. The carbon bisulphide was tested both highly purified and with sulphur in solution. The benzene had been crystallized. In no case could I find the slightest trace of rotation, though the effect of slightly turning the tube was instantly observed. The individual particles of alumina were also caused to spin violently about their own axes. The test was, in fact, a very delicate one, and the result is quite conclusive. It is to be noted that the liquids employed have by no means the same specific inductive capacity as flint glass, so that the result is not to be explained by any assumed want of discontinuity at the boundary.

Dielectric Losses in very rapidly Rotating Fields.

The ordinary electromagnetic explanation of the opacity in such substances as black pitch, bitumen, or ebonite, which, nevertheless, insulate well in bulk, is that the conductivity for small electric intensities, and over small elements of volume, may be considerable. This is, of course, not the only explanation; any cause which leads to a dissipation of electromagnetic energy might be equally cited, and amongst the most prominent of these is the possibility of electric or magnetic "hysteresis" loss. With regard to the latter, its investigation is at present beyond our powers in cases where the permeability approaches unity. I have myself, with the aid of my students, spent more than three years in trying to obtain a permeability curve for bismuth for small magnetic forces — an investigation which resulted simply in defeat. I think it quite possible, however, that experiments by a rotating magnetic field might succeed in bringing magnetic hysteresis losses to light in ordinary dielectrics — an investigation which would be easier in some respects than that with which we are now dealing, for we could reap the full advantage of the properties of quartz fibers without having to separate the effects observed from those due to accidental electrifications. With regard to electric hysteresis, however, we are more favorably situated, in consequence of the dielectric constants of ordinary transparent media being higher than the corresponding magnetic quantities. In view, however, of the effect which speed of field rotation has upon the dielectric loss, at low speeds, it is necessary to raise the speed of rotation to something more comparable with the frequencies met with in optical problems.

It has been already stated that none of the substances examined by me exhibited any dielectric loss at all at very high frequencies, and this negative result requires me to show that my arrangements were such that a loss of energy could not have been overlooked had it existed to any appreciable extent.

The only practicable method of obtaining a very rapidly rotating field is by the harmonic composition of oscillating electric forces of high frequency, and the most convenient mode of obtaining the latter is to employ oscillatory currents.

As a source of such currents I used the apparatus devised by Ebert (Wiedemann's *Annalen*, 53, 144, August, 1894). As this apparatus was exactly copied, — the author giving very full particulars, — I will merely state that by its aid it is possible to obtain oscillatory currents making about 7×10^6 complete cycles per second (*loc. cit.*, p. 152), having a wave length in air of 42 meters and a logarithmic decrement of about 5×10^{-4}. This leads to the completion of some 2000 oscillations before the amplitude falls to $\frac{1}{e}\left(=\frac{1}{2.7}\right)$ of the initial value. No attempt was made by me to verify Ebert's numbers, which may, I think, be safely taken, otherwise than by roughly calculating values for the capacity and self-induction as actually employed by me, and noting that they led with as much accuracy as could be expected to the same values as those just quoted. The diagram of Ebert's arrangement of Tesla's apparatus is included in the general diagram of connections, Fig. 3.

It was necessary for me to feed the oscillatory spark system much more vigorously than was done by Ebert. This led to the following modification: Instead of using a large Wimshurst machine, I used a very large induction coil which was formerly in the possession of the late J. E. H. Gordon, and which is profusely illustrated in his well-known work on electricity. The hammer break of this coil was replaced by a "rapid break," consisting of a disk of slate some six inches in diameter, provided

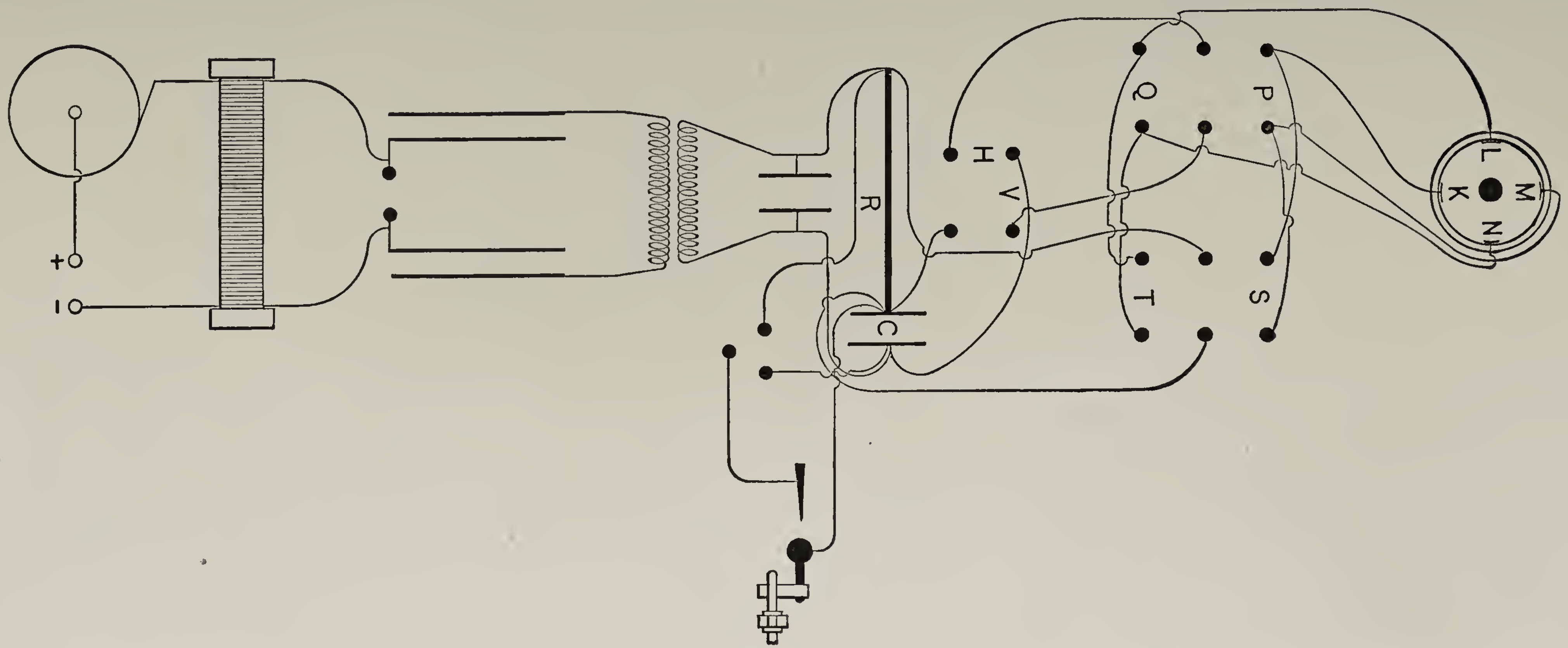

Fig. 3.

Diagram showing arrangement of Ebert apparatus, and connections between the Ebert apparatus, the phasing resistance and capacity, and the rotating field cell.

R. Phasing resistance.
C. Phasing capacity.
H, *V.* Key for reversing the direction of rotation.

P, *Q* and *S*, *T.* Two six-point keys for interchanging the connections between the armatures and phasing terminals.

with thirty copper segments against which a carbon brush was carefully adjusted. The rotating break was driven by a fan-governed water motor and provided with a contact pin enabling its speed of rotation to be measured by means of the electromagnetic apparatus of the myograph. A contact breaker requires to be very well made if it is to work regularly, and this was done in the present case. I generally used about 300 sparks per second between the primary discharge balls of the Ebert apparatus, which were 3 cm. in diameter and from 1 to 1.3 cm. apart by the shortest path. A torrent of sparks of this frequency will not give rise to oscillations in still air, but by blowing a strong air blast from a Root's blower across the spark gap, each spark becomes oscillatory. This was tested by examining the discharge by a rotating mirror, and also by examining the discharge at the secondary terminals of the Ebert appliance in like manner. The discharge of the Ebert apparatus was very vigorous, and it was necessary to round all corners of the conductors involved and to coat them thickly with paraffin.

The rotating field was set up inside a glass tube by means of four narrow armatures. At first the armatures were made of thin copper, but the Tesla flames and other effects at the edges led to their being replaced by carefully rounded pieces of aluminium, produced by flat-drawing a thick bit of aluminium wire. The armatures were coated by dipping in paraffin to a depth of about one millimeter.

The glass tube carrying the armatures formed the guard tube of the suspended system, and had a short wide side connection to a flask containing phosphorus pentoxide. The suspended apparatus required to be carefully screened as in the experiments formerly described.

There is considerable difficulty in obtaining a suitable resistance to combine with the condenser in the combination necessary to obtain the required phase difference between the potential differences of the armatures.

The condenser must be small compared with the condenser of the Ebert apparatus, or the theory of the experiment becomes unmanageable. The capacity of the Ebert appliance likewise cannot be large, or the damping becomes too great, and also the oscillations become slower. A small capacity in the phasing apparatus, however, implies a large resistance, if the potential difference at the resistance terminals is to be equal to that at the terminals of the condenser — a necessary condition. Strips of wood, graphite, etc., were tried without effect, the former exhibiting a variable resistance, the latter burning away when in sufficiently thin streaks, and hence being even more variable. I was finally compelled to use a pure water resistance. This consisted of a U tube with the two arms close together to diminish inductance; the tube was inverted thus, ∩, and the free ends dipped into two tubes full of mercury. By raising or lowering the U tube, which had a small reservoir at its upper end, the resistance could be varied. In most of my experiments a tube of about 2 mm. bore and about 20 cm. long, measured from electrode to electrode, was employed. The phasing condenser plates were 15 cm. in diameter and several centimeters apart, and were adjustable by sliding.

The equality of the maximum potential differences of the phasing combination were ascertained by a spark micrometer, a point and ball being used when it was a question of adjusting to equality, and two small spheres when the potential difference was measured. This micrometer could be thrown into either position by means of a key. In addition to this key there were three others. The first, *HV*, was used to reverse the connections of the condenser to the armatures with which it happened to be associated. The two six-point keys *PQ*, *ST*, enabled the armatures to be interchanged with respect to the terminals of the phasing apparatus. This was necessary, because the dielectric could not be centered with perfect certainty and, consequently, tended to become electrified. When this happened, small deflections made their appearance, the connection of which with the phenomenon sought required to be ascertained.

Thus the armatures *M* and *K* could be connected to the condenser, and by working the key *HV* the direction of rotation could be made either clockwise or the reverse at pleasure. The armatures *K* and *L* could then be placed upon the condenser and two fresh observations made. The substance to be examined was generally in the form of a long cylinder and was suspended by as fine a fiber as possible. The deflections were read by means of a scale at a distance of 150 cm.

It was necessary for me to have a means of estimating the armature potential differences from the observations at the spark micrometer. This would have been easy in view of Baille's and other results on spark length in air, were it not for the fact observed by Jaumann (*Wien. Bericht*, 97, p. 765), that the spark length is greatly affected by small oscillations of electric force at the surface of the conductors. Jaumann, however, does not give any rule by which the magnitude of this effect may be ascertained in any particular case such as the present, and it therefore became desirable to express the armature potential differences in terms of the potential difference of the primary spark gap in the Ebert apparatus. I was so fortunate as to secure the assistance of my friend, Professor Lyle of Melbourne, in this investigation, of which, however, I only propose to give a sketch.

Let $L = M = N$, the self and mutual inductions of the Ebert coils being numerically about 6×10^4 C.G.S.

Let $R = S -$ the resistances of the coils (about 0.088 ohm).

Let two similar circuits be arranged as in Fig. 4.

When the spark bursts across the gap in the primary circuit, consider the resistance of the gap during the oscillations as zero.

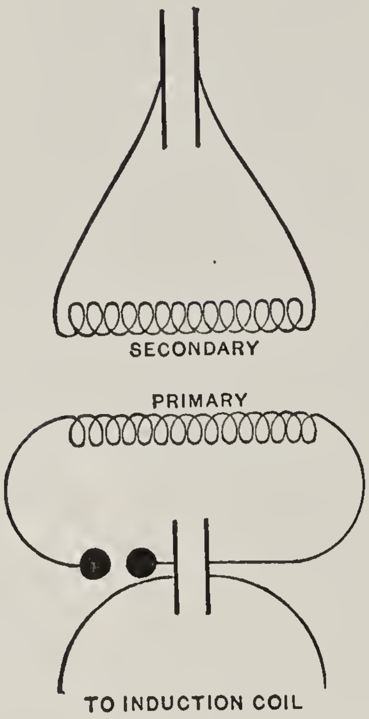

Fig. 4.

Let R be the resistance, and L the self-induction of each circuit; J the capacity of each condenser; x the charge on the primary, and y that on the secondary condenser at any moment. We have then the following equations:

$$L(\ddot{x}+\ddot{y})+R\dot{x}+\frac{x}{J}=0,$$

$$L(\ddot{x}+\ddot{y})+R\dot{y}+\frac{y}{J}=0.$$

A solution of these equations is

$$y=e^{-\frac{R}{4L}t}(A\cos\mu t \text{ and } B\sin\mu t)-\frac{x_0}{2}e^{-\frac{t}{JR}},$$

where
$$\mu^2=\frac{1}{2JL}-\frac{R^2}{16L^2},$$

and x_0 is the charge on the primary condenser when the knobs in the primary circuit are on the point of sparking.

The initial conditions

$$t=0,\ x=x_0 \text{ gives } A=\frac{x}{2},$$
$$t=0,\ y=0 \text{ gives } B=0,$$

so
$$y=\frac{x_0}{2}\left(e^{-\frac{R}{4L}t}\cos\mu t-e^{-\frac{t}{JR}}\right).$$

The second term, $e^{-\frac{t}{JR}}$, may be neglected, as it very quickly vanishes. We may take then the maximum value of y as $\frac{x_0}{2}$. The maximum value of the potential difference of the plates of the secondary condenser is therefore one half the sparking potential difference of the knobs in the primary circuit. That this is the case was verified by measuring the sparking distance between knobs. Thus, on one occasion the primary potential difference was ascertained by Baille's table to be 105, and the secondary 52 electrostatic units.

Putting $\lambda=\frac{R}{4L}$, we may write $y=\frac{x_0}{2}e^{-\lambda t}\cos\mu t$.

The values of the constants are:

$R=0.088$ ohm,
$L=6\times10^4$ electromagnetic units,
$J=1.5\times10^{-10}$ electromagnetic units;

so $\lambda=366$, $\mu=2.4\times10^7$.

In the above the capacity of the condenser used to get the phase difference is neglected, as also the capacity of the armatures.

Now consider the secondary condenser as an origin of electromotive force given by the formula,

$$E = E_0 e^{-\lambda t} \cos \mu t,$$

where $E_0 = \frac{x_0}{2J} = \frac{1}{2}$, the sparking potential difference of the knobs in the primary circuit.

Let the potentials at any instant be as marked on Fig. 5.

Assume that there is only one resistance large enough to be considered, and no inductances.

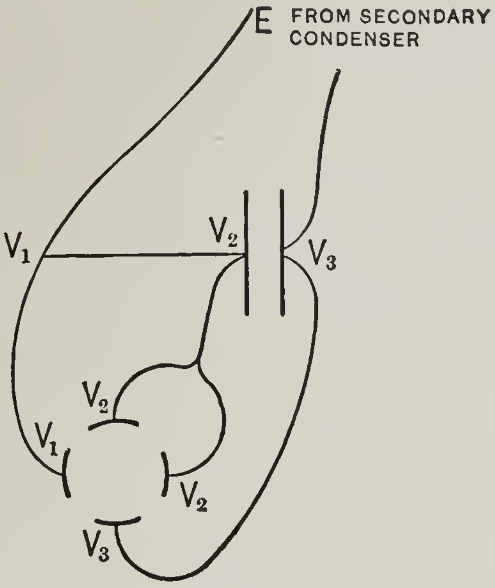

Fig. 5.

$$V_1 - V_3 = E = E_0 e^{-\lambda t} \cos \mu t.$$

$V_1 - V_2 = V\dot{q}$ ⎧ where q is the charge on small condenser and $k = \frac{1}{\text{capacity}}$
$V_2 - V_3 = kq$ ⎩ of small condenser.

Hence, $$r\dot{q} + kq = E_0 e^{-\lambda t} \cos \mu t.$$

Let $$q = e^{-\lambda t}.$$

$$\dot{q} = e^{-\lambda t}(\dot{p} - \lambda p);$$

so $$r\dot{p} + (k - r\lambda)p = E_0 \cos \mu t,$$

the solution of which may be written :

$$p = \frac{E_0}{k - r\lambda} \cos \phi \cos(\mu t - \phi),$$

where $$\tan \phi = \frac{\mu r}{k - r\lambda}.$$

Hence, $$q = \frac{E_0}{k - r\lambda} \cos \phi e^{-\lambda t} \cos(\mu t - \phi),$$

$$\dot{q} = -\frac{E_0}{k - r\lambda} \cos \phi e^{-\lambda t} \{\lambda \cos(\mu t - \phi) + \mu \sin(\mu t - \phi)\},$$

$$\dot{q} = -\frac{E_0}{k - r\lambda} \cos \phi \sqrt{(\lambda^2 + \mu^2)} e^{-\lambda t} \cos(\mu t - \phi - \psi),$$

where $$\tan \psi = \frac{\mu}{\lambda}.$$

With the values of μ and λ given, $\frac{\mu}{\lambda} = \frac{2.4 \times 10^7}{366}$, which may be considered ∞, and so $\psi = \frac{\pi}{2}$.[1] We may therefore write

$$V_2 - V_1 = -r\dot{q} = \frac{E_0}{k - r\lambda} r\mu \cos\phi e^{-\lambda t} \sin(\mu t - \phi),$$

$$V_2 - V_3 = kq = \frac{E_0}{k - r\lambda} k \cos\phi e^{-\lambda t} \cos(\mu t - \phi).$$

The maximum values of these quantities may be considered as

$$\frac{E_0}{k - r\lambda} r\mu \cos\phi, \text{ and } \frac{E_0}{k - r\lambda} k \cos\phi.$$

Experimentally either k or r can be altered until these values are equal, which occurs when $k = \mu r$.

Now $\tan\phi = \frac{\mu r}{k - r\lambda}$, so the condition $k = \mu r$ makes $\tan\phi = 1$ and $\cos\phi = \frac{1}{\sqrt{2}}$.

We have now $$V_2 - V_1 = \frac{E_0}{\sqrt{2}} e^{-\lambda t} \sin(\mu t - \phi),$$

$$V_2 - V_3 = \frac{E_0}{\sqrt{2}} e^{-\lambda t} \cos(\mu t - \phi).$$

The maximum values of these quantities may be taken as $\frac{E_0}{\sqrt{2}}$.

So the maximum potential difference across the armatures should be 0.353 times the sparking potential difference of the knobs in the primary circuit.

[*To be concluded.*]

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[1] In Recent Researches in Electricity and Magnetism, p. 295, Professor J. J. Thomson gives as the value of the impedance of a straight cable for very rapid currents

$$\left(\frac{\sigma\mu p}{2\pi a^2}\right)^{\frac{1}{2}} + \left(\frac{\sigma'\mu' p}{2\pi b^2}\right)^{\frac{1}{2}}.$$

The cable is formed of two concentric conducting cylinders separated by a dielectric. The outer radius of the core is a, and the inner radius of the inclosing cylinder, b. σ and σ' are the respective specific resistances of the conductors, μ and μ' their respective magnetic permeabilities, $\frac{p}{2\pi}$ is the frequency.

Suppose we consider that R, the resistance of either coil for steady currents in the above calculation, is replaced by $\left(\frac{\sigma\mu p}{2\pi a^2}\right)^{\frac{1}{2}} \times l$, where l is the length of the wire, in this case 1000 cm. Taking the frequency as 1×10^6 per second, the value of λ would be about 900 instead of 366; the value of μ would be practically unaltered, so $\frac{\mu}{\lambda}$ could still be considered ∞ and $\psi = \frac{\pi}{2}$.

MINOR CONTRIBUTION.

The Distribution of Alternating Currents in Cylindrical Wires.

By Ernest Merritt.

WHEN an alternating current flows along a wire the distribution of current is not uniform throughout the cross-section, as it would be in the case of a steady current. Experiment and theory both show that with alternating currents the current density is always greatest at the surface. As the frequency of alternation is increased, the difference between the current density at the surface of the wire and that at points in the interior becomes more marked, and finally, when frequencies are reached of the order of Hertz' vibrations, the current is confined to an extremely thin layer on the surface. In such cases the conductivity depends rather upon the surface of the conductor than upon its cross-section.

This so-called throttling effect has been known for many years. The fact that such phenomena might occur was indeed suggested by Maxwell. Attention was attracted to the subject by the experiments of Hughes on self-induction, and its theory has been very completely worked out by Heaviside, Rayleigh, Kelvin, and J. J. Thomson. The conclusions have also been verified, at least from a qualitative standpoint, by the experiments of Hertz, Lodge, and others.

The general interest which this phenomenon of throttling has aroused needs no better evidence than the mention of the names of the eminent men who have devoted their time to its investigation. It is, in fact, evident that the phenomenon is from several points of view an important one. In the first place, it affords an excellent illustration of the manner in which electrical disturbances are propagated, and accentuates the fact that such disturbances proceed from the ether into the wire, instead of being transmitted by the conductor itself. On the other hand, as soon as frequencies in excess of a few hundred periods per second are reached, the fact that the whole cross-section of the conductor is not utilized in the conduction of alternating currents has important practical bearings. In the case of iron wires the throttling effect, and the resulting increase in the effective resistance offered by such wires to alternating currents, become noticeable at very low frequencies.

Although, as above stated, the theory of the subject has received complete and varied treatment, numerical computations seem to have been made in only a few cases. It is generally known that this concentration of the current at the surface of a wire will occur, but it is a matter of some difficulty to find data which will enable one to tell just how great the throttling effect will be in any specified case. I have encountered this difficulty several times during the past few years, and at length decided to make the computations necessary for plotting a few curves, so as to be able to represent the results in such a form as will give a concrete idea of the nature and magnitude of the effect. The belief that the need of such numerical results has also been felt by others must serve as the excuse for this article.

In general, the computation of the distribution of current across the cross-section of a wire is so complicated as to be almost beyond the scope of analytical treatment. In a few special cases, however, the formulas involved may be reduced to a comparatively simple form. One of these cases is that of two flat conductors, so close together that they may be treated as plane conducting sheets. This case has been discussed by Lord Kelvin. Another case, and one which approaches more nearly to practical conditions, is that of a cylindrical wire at such a distance from surrounding bodies that the magnetic force in its immediate neighborhood may be looked upon as approximately the same at all points equally distant from the wire. In this case, from the symmetry of the conditions, we are authorized to assume a symmetrical distribution of current throughout the conductor, so that if r represents the distance from the center, x the position in the wire measured in the direction of the axis from some arbitrary zero point, and t the time, the current density will be a function of these three quantities only. The condition of symmetry about the axis of the wire can be accurately reached only by making the return circuit in the form of a hollow cylinder completely surrounding the wire; but in the case of a circuit formed of two wires whose distance apart is fifteen or twenty times the radius, the condition of symmetry is approached with sufficient closeness for ordinary purposes.

In discussing the theory of the subject most writers have started with Maxwell's equations for the electro-magnetic field, have modified these to suit the conditions of symmetry about an axis, and have then written down and discussed the solution of the resulting differential equation. In presenting the subject to beginners it seems to me better not to assume Maxwell's equations as already derived, but to start from the fundamental physical laws upon which these equations themselves rest, and then to apply these laws to the case in question. To obtain the differential equation by this method is perhaps in appearance not so direct as to make use of Maxwell's equations; but by referring back to the fundamental principles involved the physical side of the problem is likely to be better understood, and the process of solving the problem is less apt to be looked upon as a mere manipulation of symbols. The discussion which follows is intended as a brief outline of a method for solving the problem, which I think may be used to advantage with a class whose members are more familiar with physical principles than with the methods of physical mathematics.

In addition to the condition of symmetry about the axis of the wire, we have at our disposal for solving the problem of distribution of current the following three laws:

1. The line integral of the magnetic force about any conductor which carries a current is equal to 4π times the current. Or, as it is often stated, "the work done in carrying unit pole once around the current i is equal to $4\pi i$." This law, proven experimentally for the case of currents flowing in wires, may be extended, at least tentatively, to the case of currents in three dimensions. The fact that conclusions based upon this extension have been experimentally verified, justifies the assumption that the law applies. A mathematical expression of the law, with especial reference to the problem in hand, may be derived as follows:

Consider a small portion of the cross-section of the wire, bounded by arcs of circles whose radii are r and $r+dr$, and by the radial lines OA', OB'. If the current density is u, the total current flowing through this small area $AA'B'B$ is $ur\theta dr$.

The magnetic force at A is at right angles to OA (since the lines of force are circles about O) and has the same value at all points along AB. Let the magnetic force at A be denoted by T. T is then a function of r, and its value at points along $A'B'$ will be $T+\frac{\partial T}{\partial r}dr$. The work done by the magnetic forces when unit pole is carried around the path $AA'B'B$ is therefore

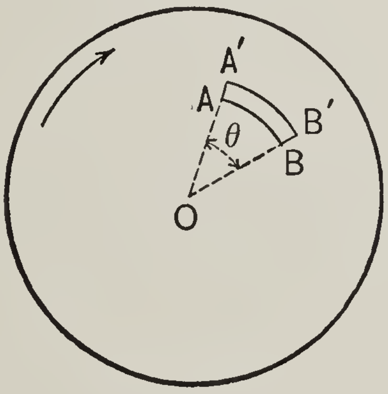

Fig. 1.

$$\left(T+\frac{\partial T}{\partial r}dr\right)(r+dr)\theta - Tr\theta,$$

and this, in accordance with the law stated, is equal to 4π times the current through $AA'B'B$. We thus have

$$\left(T+\frac{\partial T}{\partial r}dr\right)(r+dr)\theta - Tr\theta = 4\pi ur\theta dr.$$

$$\therefore\ 4\pi u = \frac{\partial T}{\partial r} + \frac{1}{r}T. \qquad (1)$$

2. The line integral of the electric intensity around any circuit is equal to the rate of decrease of the magnetic induction through the circuit. Or in other words, the electromotive force induced in any circuit is equal to the rate of decrease of the flux of induction through that circuit. This law also has received direct experimental verification only in the case of circuits of finite magnitude, but its application has been extended, tentatively as before, to the case of infinitely small circuits.

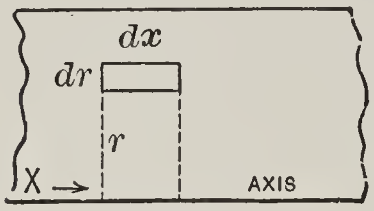

Fig. 2.

To apply it to the present case, consider a section of the wire formed by a plane passing through the axis, and determine the line integral of the electric intensity around the rectangle *drdx*. Let the electric intensity in the direction of x be denoted by P, and that in the direction of r by R; we then have

$$Rdr + \left(P + \frac{\partial P}{\partial r}dr\right)dx - \left(R + \frac{\partial R}{\partial x}dx\right)dr - Pdx = \frac{d}{dt}\cdot \mu T dr dx,$$

where μ denotes the permeability of the material composing the wire, and $\mu T dr dx$ therefore represents the total flux of induction through the area *drdx*. This reduces to the equation

$$\mu\frac{dT}{dt} = \frac{\partial P}{\partial r} - \frac{\partial R}{\partial x}.$$

If the specific conductivity of the wire is C, we have, as a consequence of Ohm's law,

$$u = CP, \quad \rho = CR,$$

where ρ denotes the component current density in the direction of r. On substituting for P and R in the equation above, we finally obtain

$$\mu C\frac{dT}{dt} = \frac{\partial u}{\partial r} - \frac{\partial \rho}{\partial x}. \qquad (2)$$

3. In the interior of a conductor there can be no accumulation of electricity; or in other words, if we consider any small portion of a conductor, the amount of electricity flowing into this portion must be equal to the amount leaving it.

Consider any small volume bounded by the cylindrical surfaces whose radii are r and $r + dr$, and by two planes perpendicular to the axis of the wire. The quantity of electricity entering this volume during the time dt is $r\theta dr u dt + r\theta dx \rho dt$. That leaving the volume by the opposite faces is

$$r\theta dr\left(u + \frac{\partial u}{\partial x}dx\right)dt + (r + dr)\theta dx\left(\rho + \frac{\partial \rho}{\partial r}dr\right)dt.$$

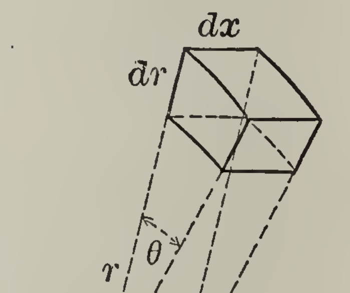

Fig. 3.

Since these quantities must be equal, we have

$$r\theta\frac{\partial u}{\partial x}drdxdt + \rho\theta drdxdt + r\theta\frac{\partial \rho}{dr}drdxdt = 0.$$

$$\therefore\quad \frac{\partial u}{\partial x} + \frac{\rho}{r} + \frac{\partial \rho}{\partial r} = 0. \tag{3}$$

We thus obtain three equations which must be satisfied by the various quantities involved, and these equations are seen to be nothing more than the analytical statement of the three physical laws quoted above. By elimination a condition is obtained which must be satisfied by the current density at all points. Elimination may be accomplished as follows:

Remembering that x and r are independent, equations (3) and (2) may be written

$$\frac{\partial}{\partial r}(r\rho) = -r\frac{\partial u}{\partial x}; \tag{3'}$$

$$-\frac{\partial}{\partial x}(r\rho) = -r\frac{\partial u}{\partial r} + \mu C\frac{d}{dt}(rT). \tag{2'}$$

If we differentiate (3′) with reference to x, and (2′) with reference to r, and add, we obtain

$$r\frac{\partial^2 u}{\partial r^2} + \frac{\partial u}{\partial r} + r\frac{\partial^2 u}{\partial x^2} - \mu C\frac{\partial}{\partial r}\frac{d(rT)}{dt} = 0. \tag{4}$$

By differentiating (1) with regard to t, we obtain

$$4\pi r\frac{du}{dt} = \frac{d}{dt}\frac{\partial(rT)}{\partial r}. \tag{1'}$$

T may therefore be eliminated between (1′) and (4), giving finally

$$4\pi\mu C\frac{du}{dt} = \frac{\partial^2 u}{\partial r^2} + \frac{1}{r}\frac{\partial u}{\partial r} + \frac{\partial^2 u}{\partial x^2}. \tag{5}$$

The distribution of the current across the cross-section must therefore be such that the current density at all points satisfies the condition expressed in the above differential equation. As it is our intention to obtain a solution only for cases where the frequency of alternation is relatively small, viz. less than 100,000 per second, the equation might be still further simplified by the omission of the term in x; for it is clear that in circuits of moderate dimensions the current will have practically the same value at a given instant at all points along the wire. This is equivalent to saying that u is independent of x, and that the terms involving x may therefore be neglected. But by omitting this term we not merely restrict (apparently) the range of application of the resulting equations, but also lose sight of the manner in which the disturbance is propagated along the wire. It therefore seems better to obtain a solution of equation (5) as it stands, and not to introduce approximations until later.

From physical considerations it is a matter of no great difficulty to determine the form which the solution of (5) must take in the present case. The experiments of Hertz have shown that if a periodic electromotive force is impressed at some point in a circuit, the disturbance will travel along the wire as a series of waves. The simplest possible expression for u would therefore be

$$u = M \cos 2\pi\left(\frac{t}{\tau} - \frac{x}{\lambda}\right),$$

where τ is the period and λ the wave length. On account of the resistance of the wire, we should expect the waves to gradually diminish in intensity as their distance from the origin increases. This requires a factor of the form ϵ^{-ax} in the expression for u. Again, the amplitude and phase of the current wave will probably depend upon the distance of the point considered from the axis of the wire. To convince ourselves of this fact, we may think of the wire as a bundle of small conducting filaments, each one of these filaments forming a branch of the circuit in parallel with all the rest. The current will then divide among the different branches in a manner depending upon their mutual and self induction. From analogy with cases of linear circuits we may at once conclude that the different currents will differ both in amplitude and phase. Returning to a consideration of the actual wire, it thus appears that the current density at any point will vary harmonically with the time, but that the amplitude and phase will be functions of the distance of the point from the center. The current density may therefore be expressed in the following form:

$$u = \epsilon^{-at}\left[M \sin 2\pi\left(\frac{t}{\tau} - \frac{x}{\lambda}\right) + N \cos 2\pi\left(\frac{t}{\tau} - \frac{x}{\lambda}\right)\right], \tag{6}$$

in which M and N are functions of r.

On substituting this expression for u in (5) and equating the coefficients of the sine and cosine terms separately to zero, two equations are obtained from which M and N can be determined. This method has been followed by Kelvin.

In this case, as in many other cases of the solution of differential equations, it is an advantage to proceed by a different method — a method which does not indicate so clearly the physics of the problem, but which makes the analytical work of solving the equation less tedious, and whose result is obtained in a form which lends itself more readily to numerical computation.

Remembering Euler's exponential expression for the sine and cosine, viz.

$$\epsilon^{-i\theta} = \cos\theta - i \sin\theta,$$

it is clear that (6) may be written in exponential form. If we write

$$\frac{2\pi}{\tau} = p,\ \frac{2\pi}{\lambda} = m,$$

we may put u equal to the *real part* of

$$S\epsilon^{-ax-i(pt-mx)}, \tag{7}$$

where S is a function, in general complex, of r. The real part of S is evidently equal to the N of equation (6), while the imaginary part of S is equal to iM. If the expression above is substituted for u in equation (5), this becomes

$$\frac{\partial^2 S}{\partial r^2} + \frac{1}{r}\frac{\partial S}{\partial r} + (4\pi i \mu Cp + (im - a)^2)\, S = 0. \tag{8}$$

The expression (7) is therefore a solution of (5) provided the function S satisfies the conditions indicated in equation (8). Since the coefficients of the original equation are all real, the real and imaginary parts of (7) must separately satisfy equation (6). We are therefore justified in using the formal solution (7) as long as there is any saving in manipulation by so doing, and may then take the real part alone as the solution of the actual physical problem.

To determine the values of a and m corresponding to any given frequency requires the dimensions and physical constants of the return circuit to be considered. A detailed discussion of several important cases has been given by J. J. Thomson.[1]

For the purposes of the present problem, however, it is not necessary to determine a and m; for both are small compared with the term $4\pi\mu Cip$. Since m involves the reciprocal of the wave length, it will be a small fraction unless the frequency is extremely great (10^8 or more per second). Experience teaches us that a, whose value depends upon the rate of damping of the waves, will also be small; p, on the other hand, will be large. The quantity $(im - a)^2$ may then be neglected,[2] and (8) becomes

$$\frac{\partial^2 S}{\partial r^2} + \frac{1}{r}\frac{\partial S}{\partial r} + 4\pi i \mu CpS = 0. \tag{9}$$

The above is a form of Bessel's equation. If we write $4\pi\mu Cp = \phi^2$, the general solution is[3]

$$S = KJ_0(\phi r\sqrt{i}) + K'Y_0(\phi r\sqrt{i}), \tag{10}$$

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[1] Recent Researches in Electricity and Magnetism, p. 262.

[2] Note that this leads to the same result as though the term in x had been omitted in equation (5).

[3] See Gray and Mathews' Treatise on Bessel Functions; or Byerly, Fourier's Series.

where K and K' are arbitrary constants. The function Y_0, however, becomes infinite when $r = 0$. This would mean an infinite current density at the center of the wire, which is evidently impossible. K' must therefore be equal to zero, and the solution reduces to

$$S = KJ_0(\phi r\sqrt{i}),$$

where J_0 is defined by the series:

$$J_0(x) = 1 - \frac{x^2}{2^2} + \frac{x^4}{2^2 \cdot 4^2} - \frac{x^6}{2^2 \cdot 4^2 \cdot 6^2} + \cdots.$$

The formal solution of equation (5) is therefore

$$u = KJ_0(\phi r\sqrt{i})\epsilon^{-ax-i(pt-mx)}. \qquad (11)$$

If we put

$$J_0(\phi r\sqrt{i}) = X - iY,$$

this becomes

$$u = K\epsilon^{-ax}(X - iY)\{\cos(pt - mx) - i\sin(pt - mx)\}.$$

As already explained, the solution of the actual physical problem is given by the *real* part of this expression. The proper expression for the current density at a distance r from the center of the wire is therefore

$$u = KX\epsilon^{-ax}\cos(pt - mx) - KY\epsilon^{-ax}\sin(pt - mx). \qquad (12)$$

Since we are concerned only with the distribution over the cross-section of the wire, any convenient value, as zero, may be assigned to x. If in addition we make the maximum value of u at the center of the wire equal to unity, the expression for the current density may be put into the following form:

$$u = X\cos pt - Y\sin pt,$$

or

$$u = u_0\cos(pt + \theta),$$

where

$$u_0 = \sqrt{X^2 + Y^2},$$

$$\tan\theta = \frac{Y}{X},$$

and

$$X - iY = J_0(\phi r\sqrt{i}) = J_0(\sqrt{i}\sqrt{4\pi\mu Cpr^2}).$$

It is thus seen that the maximum value of the current density, and also the phase of the current at any distance r from the axis, depends upon X and Y only; while X and Y in turn depend only upon the value of $4\pi\mu Cpr^2$. In recording the results of numerical computation it is therefore convenient to tabulate the values of u_0 and θ corresponding to a series of values of the expression $4\pi\mu Cpr^2$. By giving proper values to the constants μ, C, p, and r, such a table may be made to give numerical results for any special case within its range. The values of $J_0(q\sqrt{i})$ up to $q = 6$

are given by Gray and Mathews,[1] while a table extending, with greater intervals, to $q = 20$, has been computed by Mr. Magnus Maclean.[2] By the aid of the values for X and Y given by these writers, I have computed u_0 and θ for different values of q, where $q = 4\pi\mu Cpr^2 = 8\pi^2\mu Cnr^2$, n being the frequency. The results are given in the accompanying table.

Table I.

q	X	Y	u_0	θ (in degrees).	$\frac{R_n}{R_s}$
0.	1.000	0.000	1.000	0.	1.000
0.5	0.999	0.062	1.001	3.5	1.000
1.	0.984	0.250	1.015	14.3	1.000
1.5	0.921	0.558	1.077	31.2	1.026
2.	0.752	0.972	1.229	52.3	1.080
2.5	0.400	1.457	1.511	74.6	1.175
3.	– 0.221	1.938	1.96	96.5	1.318
3.5	– 1.194	2.283	2.58	117.6	1.492
4.	– 2.563	2 293	3.44	138.1	1.678
4.5	– 4.299	1.686	4.62	158.6	1.863
5.	– 6.230	+ 0.116	6.23	178.9	2.043
5.5	– 7.973	– 2.790	8.45	199.3	2.219
6.	– 8.858	– 7.335	11.5	219.6	2.394
8.	+ 20.97	– 35.02	40.8	300.6	3.096
10.	– 138.8	+ 56.37	150.	382.1	3.794
15.	– 2970.	– 2952.	4190.	584.8	5.573
20.	+11500.	+47580.	48900.	796.4	7.325

Explanation of Table.

$q = \sqrt{4\pi\mu Cpr^2} = \sqrt{8\pi^2\mu Cnr^2}$, where μ is the permeability, C the specific conductivity, and r the distance of the point considered from the center of the wire. n represents the frequency. Then u_0 is the value of the maximum current density, the current density at the center being taken equal to unity. θ is the phase difference between the current at the distance r from the center and the current along the axis of the wire.

$\frac{R_n}{R_s}$ is the ratio between the resistance offered to alternating currents of frequency n, and the resistance offered by the same wire to steady currents.

$$X - iY = J_0(q\sqrt{i}).$$

As an illustration of the use of this table, consider the following special case: A copper wire 1 cm. in diameter carries an alternating current whose frequency is 2000 per sec. How does the current density at the surface compare with that at the center?

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[1] Treatise on Bessel Functions, Table IV.

[2] See Kelvin, Mathematical and Physical Papers, Vol. III., p. 493.

We have in this case,

$$\mu = 1,\ C = \frac{1}{1600},\ n = 2000,\ r = 0.5,$$

$$q = \sqrt{8\pi^2 \mu C n r^2} = 4.97.$$

The value of u_0 corresponding to $q = 4.97$ is very nearly 6.1. The maximum current density at the surface of the wire is thus six times as great as the maximum value at the center. The phase difference θ is in this case nearly 178°. It thus appears that the current at the center of the wire lags behind the current at the surface by almost 180°; so that when the surface current is at its maximum in the positive direction the current at the center of the wire has just reached its maximum in the negative direction.

By giving different values, ranging from 0 to 0.5, to r, the value of u_0 may be found for all points in the wire. The dotted curve in Fig. 4 has been plotted from results obtained in this way, distances from the center of the wire being used as abscissas, and the corresponding values of u_0 as ordinates. To avoid interpolation, n has been taken equal to 2020, thus making $q = 5$ at the surface.

While this curve indicates fairly well the magnitude of the throttling effect, it fails to give an entirely satisfactory picture of the actual condition of affairs in the wire. On account of the varying phase angle θ, the current density does not reach its greatest value at the same instant at all points of the cross-section, so that the dotted curve in Fig. 4 does not show the actual distribution of current at any definite time. To find the distribution at any given instant, we must assign the proper value to u_0 and θ in the expression

$$u = u_0 \cos\left(\frac{2\pi t}{\tau} + \theta\right),$$

and thus determine u for different values of r, but for the *same* value of t. If these results are plotted, we obtain what might be termed an instantaneous view of the current distribution. Three such curves are shown in Fig. 4. The first shows the condition of affairs at the instant that the surface current has reached its positive maximum. At a distance of 2.8 mm. from the center of the wire the current at this instant is zero, while at all points still nearer the axis the current flows in the opposite direction. It is clear that so far as outside magnetic effects are concerned, the current in the interior of the wire tends to neutralize the effect of the surface current. In this case the presence of the interior portion of the conductor is not merely of small utility; it is actually a disadvantage. The second curve in Fig. 4 gives the distribution at the instant that u at the surface is passing through zero in the negative direction. In Curve III the surface current is approaching its negative maximum.

For a frequency of 2000 the values of θ are too small to bring out clearly the manner in which the current is propagated into the wire. By taking $n = 8080$, which makes $q = 10$ at the surface of a copper wire 1 cm. in diameter, we obtain a case where all the phenomena of throttling are much more marked. The maximum current density at the surface is now 150 times as great as that at the axis, while the phase difference between the two currents is 382°. The variation of u_0 with r in this case is shown in Fig. 5, IV. The dotted curve in the same figure gives the value of θ at different distances from the axis. It will be observed that the latter curve, with the exception of the region near the center, is practically straight. We conclude from this that the current density at points not too near the axis lags behind the surface current by an angle which is proportional to the depth below the surface. The disturbance which constitutes the current thus travels inward from the surface of the wire in the form of a wave, whose velocity of propagation remains practically constant until the wave has almost reached the center. The amplitude, on the other hand, diminishes very rapidly as the depth below the surface increases. The curves plotted in Fig. 5 have been drawn to illustrate this point. In Curve I we have the distribution at the instant when the surface current is passing through zero in the positive direction. Curve II shows the condition

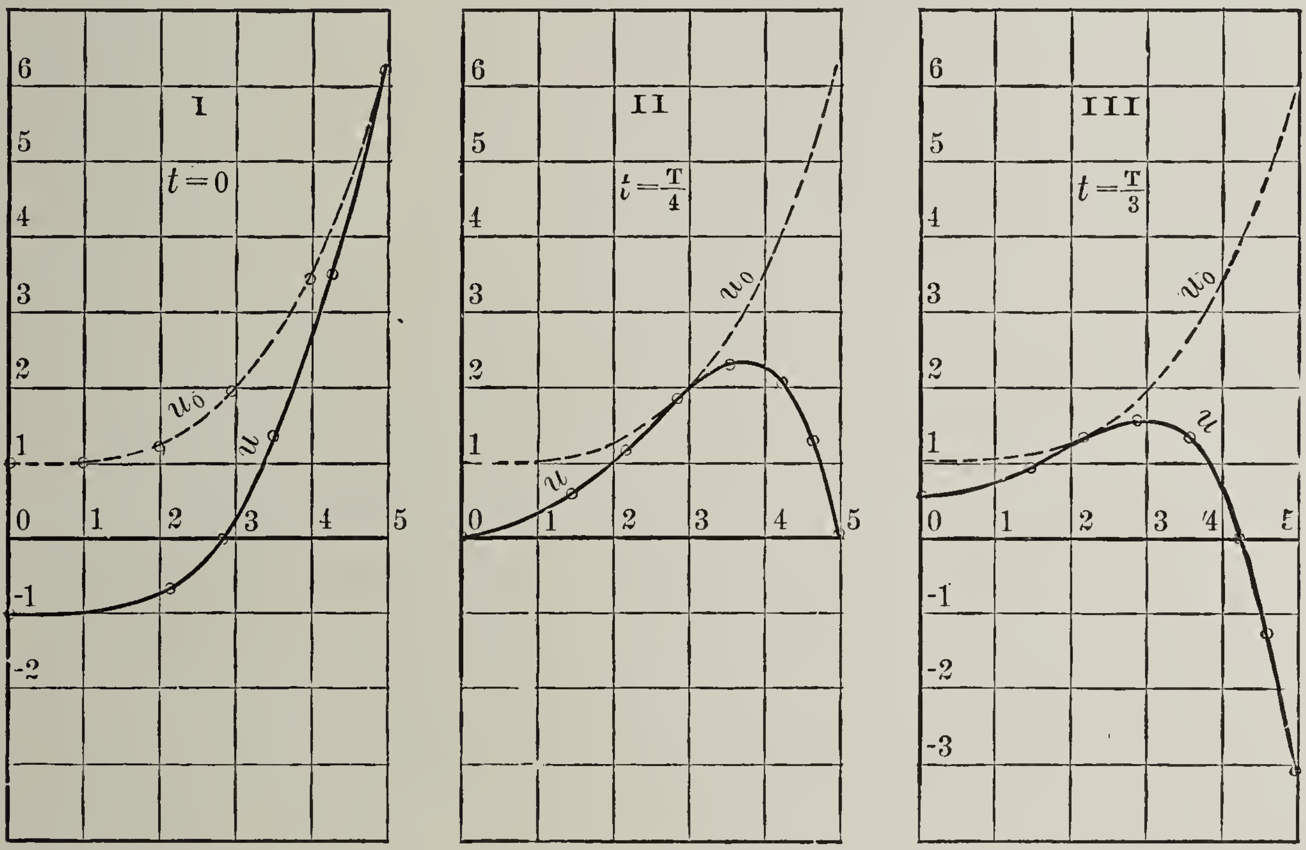

Fig. 4.

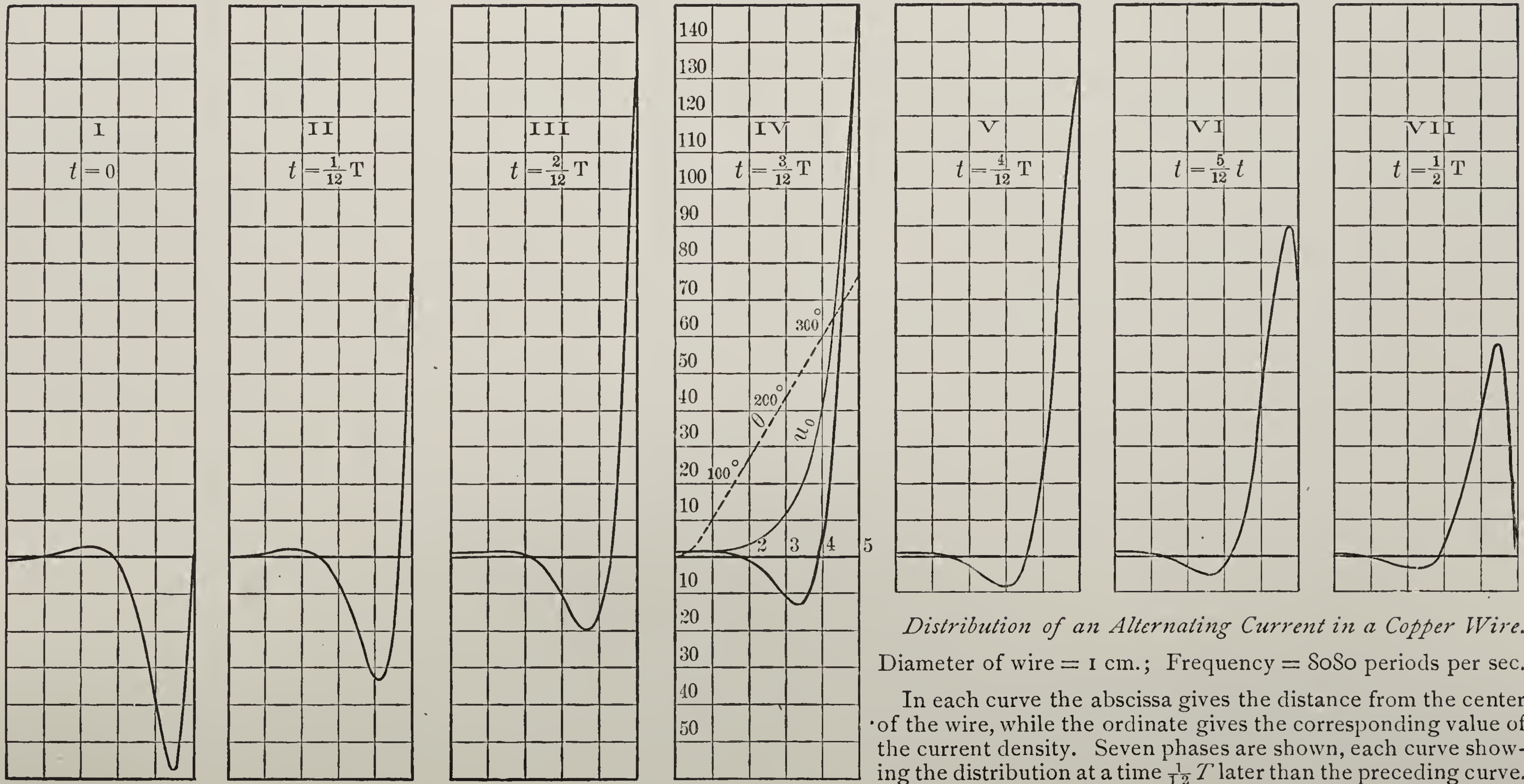

Fig. 5.

Distribution of an Alternating Current in a Copper Wire.

Diameter of wire = 1 cm.; Frequency = 8080 periods per sec.

In each curve the abscissa gives the distance from the center of the wire, while the ordinate gives the corresponding value of the current density. Seven phases are shown, each curve showing the distribution at a time $\frac{1}{12}T$ later than the preceding curve.

of affairs $\frac{1}{12}$ period later; Curve III gives the distribution at the end of another $\frac{1}{12}$ period interval; and so on. Figure 5 thus shows seven successive phases of the disturbance in the wire, and enables the phenomenon to be followed through half an oscillation. It may be added that the next half oscillation will differ from that shown only in the fact that the sign of u is reversed.

Only a brief inspection of Fig. 5 is necessary in order to appreciate the manner in which the current is propagated into the wire. The wave of current, starting in Curve I near the surface of the wire, may be followed through its successive stages as it proceeds, with rapidly diminishing amplitude, toward the axis. These diagrams also enable the velocity of propagation to be determined. If we compare Curves I and VII, we see that the zero point of the current wave has moved from $r = 5$ mm. to $r = 2.82$ mm. The wave has therefore traveled a distance of 2.18 mm. in $\frac{1}{16160}$ sec. This corresponds to a velocity of 35 meters per second.

In general, the velocity with which the disturbance is propagated inward will depend upon the frequency of the current as well as upon the material of the wire. The velocity will not be strictly constant unless the diameter of the wire is infinite; but for points in the wire for which q exceeds 2 it will be so nearly constant that its variation is negligible. A general expression for the velocity is

$$V = \sqrt{\frac{n}{\mu C}}.$$

This expression is obtained on the assumption that $r = \infty$; *i.e.* for a plane conducting sheet.[1] That it holds with a high degree of approximation even for rather small wires may be verified by the use of the table given above.

There results from this concentration of current at the surface a real change in the resistance of the wire — real in the sense that it corresponds to an increased development of heat, and not merely to a change in the impedance of the circuit. The central portion of the wire no longer contributes its share to the conductivity, so that the resistance is practically that of a thin cylindrical tube. In the last column of the table on p. 55, I have tabulated the values given by Kelvin[2] for the effective resistance offered by cylindrical wires to alternating currents. The resistance R_n for an alternating current of frequency n is here expressed in terms of the resistance R_0, which the same wire offers to steady currents. In Fig. 6 these results are shown graphically for the case of a copper wire 1 cm. in diameter.

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[1] See Kelvin, "Anti-effective Copper," London Electrician, Vol. 25, p. 512, 1890.

[2] Mathematical and Physical Papers, Vol. III., p. 492.

On account of the high permeability of iron the various effects of throttling are shown by wires of that material even when the periodicity is quite low. If we assume a permeability of 1500, which is a low value for soft iron, the curves of Fig. 5 would apply to an iron wire 1 cm. in diameter when the frequency is only 32 per second. In the case of the magnetic metals, however, the value of μ cannot be treated as constant. The permeability in such cases is a function of the current, and, if hysteresis is appreciable, of the rate of change of the current also. The results derived above can therefore not be expected to show an exact agreement with experiment in the case of iron wires, although they doubtless give a rough approximation to the truth. The subject of the distribution of alternating currents in wires of the magnetic metals is in fact one which deserves further experimental study. It can hardly be doubted that such an investigation would be a valuable contribution to our knowledge of magnetism.

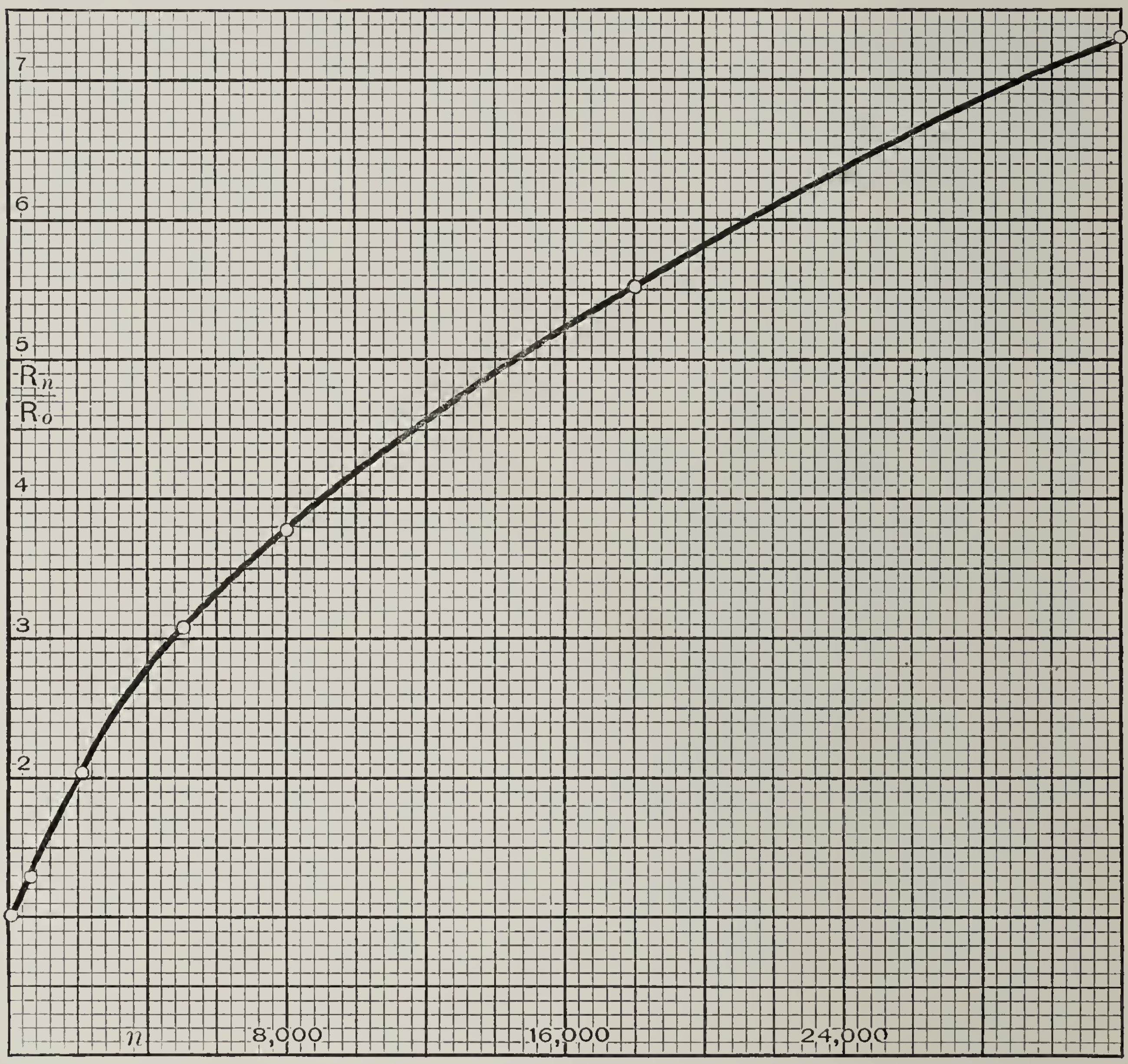

Fig. 6.

NEW BOOKS.

What is Electricity? By John Trowbridge, S.D. (The International Scientific Series, No. 75.) New York, D. Appleton & Company, 1896.

It is a little singular that this well-known series should have reached its seventy-fifth number without including a volume on any electrical subject. No less than five have been devoted to various branches of optics, and several to other departments of physics. Professor Trowbridge's book, though the first on this topic, can hardly be considered as an introductory treatise. It reviews the whole field of electrical phenomena — and many other things besides; but it aims to present them in such a way "that the reader can perceive the physicist's reasons for supposing that all space is filled with a medium which transmits electromagnetic waves to us from the sun."

The treatise indeed, as the author states in another place (p. 4), is intended to be "a popular presentation of what I regard as the real subject of physics, — the transformations of energy, — and of the greatest generalization in that subject (Maxwell's Theory of Electromagnetic Origin of Light and Heat)."

While the theme is highly attractive, it is not an easy one for popular treatment, because of the large amount of preliminary information which is necessary to any really definite conception of electrical oscillations.

The essence of the book lies in the later portions. In them, from the chapter on alternating currents, are discussed successively Leyden-jar discharges, lightning, step-up transformers. Then the author, drawing nearer the heart of his theme, describes the high-frequency experiments of Tesla and Elihu Thomson, passes to the subject of electrical waves, and the work of Hertz, Lenard, and Röntgen, and, after a short discussion of the electromagnetic theory of light, closes with a résumé of electrical theories in general.

These chapters, to him with sufficient knowledge to understand them, open the way into a new and fascinating field of physical research. The experimental illustrations are striking and ingenious. The phenomena described have not hitherto been grouped together in one treatise, and many of them have been almost inaccessible to the general reader. The very richness of the subject, however, has brought its own difficulties. The amount of ground gone over is so great that the explanations are often hasty and inadequate, sometimes even incorrect. In the chapter on wave motion — to give a single instance — is brought together a great number of curious and interesting experimental results, so briefly described as to be practically unintelligible to a reader without a previous knowledge of physics. Here within the compass of two dozen duodecimo pages are discussed Wiener's photographs of standing waves in a film, Lippmann's color photographs, Boys' photographs of flying bullets, Töpler's method of rendering sound waves visible, Langley's bolometric researches, and many others. Michelson's interference apparatus is described in six lines, and the description is as erroneous as it is short.

The first eight or ten chapters treat of the more ordinary and well-known electrical manifestations, and must be regarded as introductory to the subject proper. The extremely elementary nature of some of them — such as those on the galvanometer and dynamo — shows that no previous knowledge of electricity is assumed on the part of the reader. To these early chapters he must look for whatever principles and definitions he may need to comprehend properly the portions which follow, yet he will look in vain for definite information upon many important topics, which are referred to elsewhere, without explanation.

More to be regretted than even such omissions, are the half-truths, misleading statements, and errors, which are numerous.

The "absolute system" of measurements is explained (p. 11) as "absolute in the sense that everything is referred to acceleration of a body falling to the earth at a definite place through a certain space in a definite time." This is not easily reconciled with the ordinary conception of an absolute system, which is explained by Andrew Gray as "a system of fundamental units in no way affected by locality or other conditions of experimenting." The definitions of electrical units (p. 62) are unintelligible as they stand, though of course the meaning is clear enough to one who already knows it.

It is at least misleading to say that a "compass needle instantly points to the wire carrying the current" (p. 47), or that magnetic forces are proportional to the products of the attracting "masses" (p. 107).

It is said of polarizing bodies — a Nicol prism is instanced as an example — that "we can think of such substances as similar in internal construction to a Venetian blind with its horizontal slats" (p. 80). Of course we *can* think so of it if we like, but it is hardly fair to leave upon the trusting reader's mind the impression that the Venetian blind is anything more than a crude illustration. Even less excusable is the explanation of diffraction through a slit, that the bright and dark lines extending each side of the slit "are due to the waves of light which emanate from the illuminated edges of the slit, and which interfere" (p. 223).

Statements of a similar character are to be found in the description of the diffraction grating (p. 230) and of the Rowland concave grating (p. 232). Many others might be cited. The beginner who takes this book for a guide will have much to unlearn, should he ever wish to carry his reading further.

The question which forms the title of the book is nowhere directly answered, but in several places statements are made which seem to throw light on the author's opinion.

On p. 49 it is said that "the only difference between light, heat, and electricity is in the length of waves," and on p. 105, "Our principal source of electricity to-day is the steam-engine; yet it can be maintained that electricity is back of the steam-engine. It is in the coal" — and again (p. 128), "Nature sends us, so to speak, our electricity by means of to-and-fro motions from the sun." In several places also the "velocity of electricity" is said to be the same as the velocity of light.

Judging from these statements, Professor Trowbridge holds electricity to be a form of energy, which, in the present state of electrical theory, is a hard saying.

Frank P. Whitman.

James Clerk Maxwell and Modern Physics. By R. T. Glazebrook, F.R.S. (The Century Science Series.) New York, Macmillan & Company, 1896.

The life and scientific work of James Clerk Maxwell has already been so admirably presented that a new biography might seem superfluous. There is little indeed to be found in the first portion of the present volume, which is biographical in its character, that has not appeared either in the work of Campbell and Garnett,[1] or the preface to Maxwell's Collected Works,[2] by Niven.

Professor Glazebrook, than whom no one is better entitled to speak of Maxwell, has, however, gathered together, within the compass of less than a hundred pages, materials which give a very vivid and altogether pleasing picture of Maxwell's short but extraordinary career; and his sketch, because of its brevity, will doubtless be read by many who would never have opened the larger work of Campbell and Garnett.

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[1] *The Life of James Clerk Maxwell,* by Lewis Campbell and William Garnett, London, Macmillan & Co., 1882.

[2] *Scientific Papers of James Clerk Maxwell,* edited by W. D. Niven, Cambridge, 1890.

The latter half of the volume is devoted to a *résumé* of Maxwell's work. To give any adequate expression in popular language to the results of profound investigations, such as those upon which the fame of Maxwell rests, is an exceedingly difficult task. This task has been admirably done. Those who are acquainted with the researches in their entirety will read this lucid presentation of them with great pleasure; while every intelligent reader will be able to gain a definite conception of the general scope of Maxwell's contributions to science.

E. L. N.

Humphry Davy, Poet and Philosopher. By T. E. Thorpe, LL.D., F.R.S. (The Century Science Series.) New York, Macmillan & Company, 1896.

Chemistry has made such strides since the beginning of the century that an intelligible account of the labors of Sir Humphry Davy was much needed. Professor Thorpe's little book gives a more accurate account of the man, and of his work, than can be derived from the reading of the biographies written by Paris and by John Davy, or from the sketch contained in Brougham's *Lives of Men of Letters and Science.*

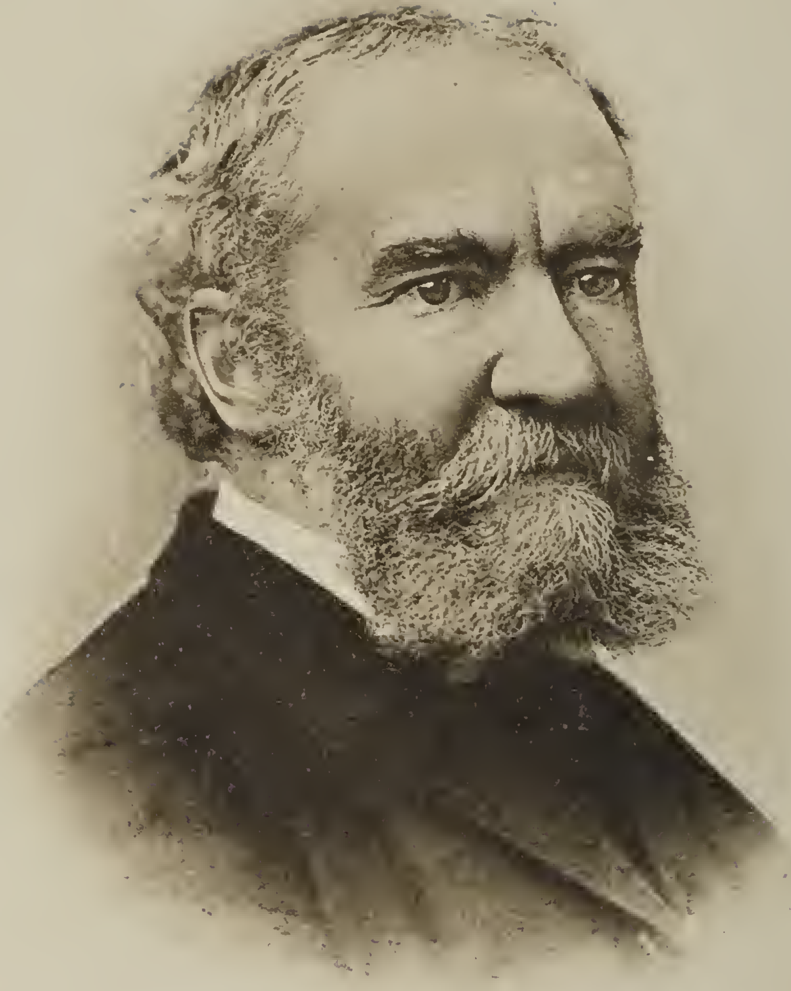
It is easier for the writer to-day to estimate the value of Davy's labors, and to obtain an impartial view of his character and motives, than it was for those who were his contemporaries. As to the place which should be assigned to his work in the history of the development of physics and chemistry, it was indeed impossible to speak impartially, until the entire era to which his labors belonged had become historical. In this still somewhat delicate matter Professor Thorpe has been very skillful. His account of Davy, the man, is appreciative and discriminating, and at the same time quite free from any elements of hero worship. In his description of Davy's discoveries he has made many points, otherwise obscure, intelligible to those to whom the terminology of that great transition period in chemistry, the beginning of the century, is unknown.

E. L. N.

NOTICE: CHANGE FROM ANNUAL TO SEMI-ANNUAL VOLUMES.

Beginning with the present number, two volumes of the Physical Review will be published annually. These volumes will begin in July and January respectively, and will each contain at least five numbers.

The past volumes of the Physical Review (I.–IV.) are annual volumes, each containing six bi-monthly numbers, beginning with the July-August number.



Alfred M. Mayer

Volume V. August, 1897. Number 2.

ELECTRICAL ENGINEERING DEPARTMENT
University of Illinois.

THE

PHYSICAL REVIEW.

ON THE CONVERSION OF ELECTRIC ENERGY IN DIELECTRICS.[1] III.

(*Conclusion.*)

By Richard Threlfall.

IN a particular test the primary potential difference was 105 electrostatic units, and the potential difference across the armatures, as tested by sparking distance, only 10 electrostatic units, instead of 37, as it should be according to this theory. The difference is to be explained by losses from brush discharges and imperfections in the theory.

If we consider the electric force at the center of the armatures as made up of components X and Y at right angles to each other, we may write

$$X = \frac{V_2 - V_1}{d},$$

$$Y = \frac{V_2 - V_3}{d},$$

where d is the distance between opposite armatures. We may write, then,

$$X = Ae^{-\lambda t} \sin (\mu t - \phi),$$

$$Y = Ae^{-\lambda t} \cos (\mu t - \phi),$$

where

$$A = \frac{E_0}{\sqrt{2} \cdot d}.$$

The field between the armatures is therefore a rotating one, and the value of the electric force at the center can be represented by the radius vector of a spiral, the magnitude ρ of the radius vector at any time being given by

$$\rho = Ae^{-\lambda t}.$$

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[1] Continued from the Physical Review, Vol. V., p. 46.

Now
$$\int^{\infty} \rho dt = \frac{A}{\lambda},$$
and if T is the interval between two sparks across the knobs in the primary circuit, the mean value of the force will be $\frac{A}{\lambda T}$.

Putting in the value of A, we get
$$\text{mean force} = \frac{E_0}{\sqrt{2}\, d\lambda T}.$$

With respect to the above investigation, it is to be noted that it is tacitly assumed (1) that the currents are distributed uniformly throughout the conductors, and (2) that the conductors are short compared with the wave length. An idea of the error introduced by the former supposition is to be obtained from the previous footnote: it practically gives rise to no appreciable difference. With regard to the latter, all the conductors are short compared with the wave length, except the coils of the transformer of the Ebert apparatus. This does not give rise to any appreciable error, however, for I found the secondary potential difference almost exactly what it ought to have been.

With regard to the effect discovered by Jaumann, relating to the influence of a small impressed oscillatory potential difference at the spark gap, we may note that in the present case, either in the primary or in the secondary, the potential difference rises rapidly, but still gradually, to its maximum value, — a state of affairs which does not correspond with that treated experimentally by Jaumann. Other observers, however, have indicated that oscillatory discharges of very high frequency take place more easily than slower ones; but I am not able to discover how far the observations refer to the first spark as distinguished from those following it at very small intervals of time. Suppose, however, that such an effect really exists and operates on the first maximum in the armature (phasing) terminals. The result would be still further to increase the difference between the observed and calculated potential differences. Now the observed potential difference, as deduced from "steady spark length," was about one-quarter of the calculated potential difference. It will be presently shown that no reasonable reduction of the value thus obtained could possibly account for the absence of any observable rotation in the cases examined, assuming that the losses are the same at these high frequencies as they are at lower speeds. As an illustration of the method of making an experiment in the rapid field, I will select for description some observations on an ebonite cylinder, made from the same kind of ebonite as was examined in the slower fields. A fine thread having been selected, the ebonite cylinder was mounted and dried for several days by phosphorus pentoxide. The Ebert apparatus was fed by sparks from the large induction coil at the rate of an interval of 0.0014″ between each spark. The sparks were examined by a revolving mirror, and the resistance of the primary of the induction coil was so adjusted as to allow the sparks to pass freely. It resulted from a previous experiment that the sparks only occurred on the make and not on the break of the primary circuit. The spheres of the Ebert apparatus were 3 cm. diameter of heavily gilt and polished brass. The rotating mirror (hand turned) disclosed that each spark was multiple. Since the period of oscillation was too rapid for the separate oscillations to be separated by the mirror at the speed at which it was driven, it was inferred that the enormous resistance, self-inductance, and capacity of the secondary of the induction coil was giving rise to discontinuous discharge in the well-known manner. The period quoted is merely that of the spark groups, or breaks of the induction coil circuit; consequently it appears reasonable to infer that the number of effective sparks was about four times as great as the number resulting from the period quoted. We are, however, in search of a minimum possible value for the intensity in the rotating field, and consequently the sparks will be reckoned at one only per break of primary. The spark length between the balls of the Ebert apparatus was 1 cm., corresponding to 105 electrostatic units potential difference.

The spark length between 1 cm. balls attached to the secondary condenser was such as to indicate a potential difference exactly half of this, within the limits of errors of observation. The potential differences were deduced by a reference to Baille's results (J. J. Thomson, *Recent Researches in Electricity and Magnetism*, p. 77).

The phasing condenser plates were 15 cm. diameter, and when the phasing potential differences were equal they were 2.3 cm. apart. When the rotating field cell was connected with the wires and keys, readjustment was obtained by keeping the resistance constant and increasing the plate distance to 3.1 cm. This shows that the capacity of the cell was by no means to be neglected in reality, though we are more or less forced to neglect it in our calculations. This disturbance constitutes the most serious objection to the interpretation of the results, but it is not, after all, very large. The potential difference of the phasing terminals, with cell attached, was tested by observing the spark length between spheres 0.4 cm. diameter. One of these spheres was mounted on the slide of a measuring microscope (with, of course, the necessary insulation), and was found to yield a spark length of 0.06 cm. This corresponds to about 10 electrostatic units of potential difference. When the cell was disconnected, the potential difference rose so as to give a spark length of 0.14 cm., the difference being attributable to the inevitable loss by brush discharge, which it is practically impossible to avoid with the complexity of wires and keys actually employed.

Calculating the mean force by help of the formula arrived at in the mathematical discussion, we have

$$F = \frac{E_0}{\sqrt{2}\, d\lambda t}.$$

The data, besides those already given, used in the calculation of λ, are:

$E_0 = 105$ (electrostatic). $\lambda = 366$.
$t = 14 \times 10^{-4}$ seconds. $d = 1.3$ cm.

The resulting value of F is about 56 electrostatic units. Applying an "internal screening factor," this reduces to about 16.8 electrostatic units actually active in the mass of the cylinder of ebonite.

But we have seen that the want of uniformity in the distribution of the currents in the conductors may increase λ rather less than three times. Hence, taking $3\,\lambda$ instead of λ, the external force becomes 18, say, and the internal force, say, 5.

But the observed external force is $\frac{10}{1.3} = 7.7$, say, instead of 18, and the resulting external force is 2.3, say.

Using these latter numbers, we have the energy loss per unit volume by the previous investigation in slow fields

$$W = 0.03\, F^{1.85},$$

using the formula applicable to the smaller sphere. In this case the volume of the ebonite cylinder was 0.36 cc., so that the energy expended and lost becomes 0.4 erg per revolution. This implies a possible rise of temperature sufficient to melt paraffin in, say, about ten minutes, but the ebonite was cemented on by paraffin, and the field was often in action for ten minutes and more, but the ebonite never fell off. It is quite a question whether a temperature method might not yield instructive results if used to check the dynamometric method. Of course, as the apparatus was arranged, no temperature tests were possible. In order to compute what angular displacement of the cylinder is to be expected, the following data are necessary.

Time of double vibration of cylinder of moment of inertia, 0.08 (g.cm.^{2}); with thread employed = 43″, hence coefficient of torsion of thread is $1.7 \times 10^{-3} = \tau$, and if ϕ is the angular rotation of the cylinder in radians, we have $2\pi\tau\phi = W$.

This gives, with above minimum value for W,

$$\phi = 37 \text{ radians}.$$

Now, as the scale was set up at a distance of 150 cm., a 5 cm. deflection, which I shall show directly could not have been overlooked, would correspond to a rotation of the ebonite of 0.016 radian.

I will now show that though low frequency results would lead to an angular motion of the light spot of 74 radians, the observed angular motion was either absent or less than 0.032 radian; that is, $\frac{1}{2312}$ of the deflection which would have been observed had the low frequency loss persisted.

There was some fear that the water resistance might introduce a sensible capacity at the terminals, and consequently, in this case as in all others, three experiments were made.

(1) With the resistance and capacity in the phasing arrangement as described.

(2) With the resistance augmented by a small impedance, viz. the coil of a low resistance galvanometer.

(3) With two dead resistances of water — one the resistance employed in the other arrangements, and the second adjusted to equality by adjusting the spark lengths.

The results of many sets of measurements are collected for reference in Table A.

It may reasonably be inferred from the irregularity of the results that the deflections, such as they were, were the result of unequal electrifications of the ebonite, and this is supported by the fact that the deflections were not generally produced with the suddenness which should have characterized the real effect when the coil was suddenly started. On several occasions, also, observations were interrupted by the cylinder becoming electrified permanently to such an extent as to throw the reading right off the scale, where it remained long after the coil was stopped.

It may, perhaps, be urged that owing to the capacity of the water resistance, or of the leads, or of the armatures, a phase difference of 90° was not in reality set up. This is, of course, possible, but it is hardly likely that in the three cases examined the phase difference would have been 0° or 180° in each case, and yet this is the position we must take up if we assume that no deflection was observed, because in fact the field did not rotate. In fine, the margin is so great that we may admit that the vibration was nearly plane polarized in all three cases without affecting the conclusion.

Similar experiments were performed on flint glass and on sulphur, containing a great deal of the insoluble variety, and therefore suitable for the detection of any effect, but the results with both these substances were practically identical with the results as quoted.

These experiments were made before the unique properties of selenium had been discovered, and the apparatus was in use for other purposes, or I should, most certainly, have examined this curious substance.

We may now summarize the results of this investigation in Tables B and C.

Table A.

EBONITE CYLINDER IN HIGH FREQUENCY FIELD.

Clockwise rotations marked +; counter-clockwise marked —.

Position of the two six-point keys.	Position of the rotation reversing key.	Expected direction of rotation. Clockwise marked +.	1 Deflections in centimeters. Phasing, condenser, and resistance adjusted.	2 Deflections in centimeters. Two equal resistances in phasing apparatus.	3 Deflections in centimeters. Phasing as in column 1. Normal	4 Deflections in centimeters. Inductance added to resistance and condenser.	5 Deflections in centimeters. Resistance, induction, and condenser, as in 4.	6 Deflections in centimeters. Two resistances.	7 Deflections in centimeters. Resistance and capacity. Normal.	8 Deflections in centimeters. Resistance, inductance, and capacity.	9 Deflections in centimeters. Two resistances only.	10 Mean deflections in centimeters. Normal arrangement.	11 Mean deflections in centimeters. Resistance, inductance, and capacity.	12 Mean deflections in centimeters. Two resistances.
A	*V*	—	0.0	+ 2.5	+ 0.8	0.0	— 2.0	— 1.0	— 2.0	— 1.0	— 1.0	— 0.62	— 0.1	+ 0.2
A	*H*	+	—13.2	—13.5	—11.6	—18.0	—25.0	—20.0	—17.0	—16.0	—15.0	—14.0	—20.0	—16.0
B	*H*	—	+21.5	+21.0	+20.0	+35.0	+20.0	+14.0	+13.0	+12.0	+10.0	+18.1	+22.0	+15.0
B	*V*	+	0.0	— 0.6	+ 0.3	— 1.5	— 1.5	— 2.0	— 1.0	— 1.2	— 1.4	— 0.2	— 1.4	— 1.3

Table B.

Description.	Energy loss and electric intensity relation.	Percentage loss $F = 1$.	Percentage loss formula.	Frequency.	Effect of change of frequencies.
Ebonite "Long *A*"	$W = 0.029\ F^{1.765}$	21.2	$21.2\ F^{-0.235}$	0.15	Very slight increase with speed.
Silvertown best.					
Large sphere	$W = 0.017\ F^{1.85}$	—	—	—	Nil.
Smaller sphere	$W = 0.03\ \ F^{1.85}$	—	—	—	
Flint glass spheroid	$W = 0.038\ F^{1.92}$	14	$14\ F^{-0.08}$	18.8	Nil.
Sulphur spheroid:					
A, aged monoclinic; not especially dried	$W = 0.0139\ F^{1.91}$	11.05	$11.05\ F^{-0.09}$	22	Large decrease.
C, dried	Index $n = 1.5$	Too small for measurement			
K, 0.7 per cent insol.: perfectly dry	$W = 0.00048\ F^{1.857}$	0.0038	$0.0038\ F^{-0.143}$	18	Small increase.
Paraffin M.P. 50°:					
Spheroid *A*	Too small to measure by thread used for ebonite				
B, M.P. 52°	Very small loss indeed Index 1.95				
C	$W = 0.00138\ F^{1.56}$ $W = 0.0008\ F^{1.56}$	1.51 0.88	$1.51\ F^{-0.44}$ $0.88\ F^{-0.44}$	12.5 27.3	Considerable decrease.
Various spheres	Index 1.5 to 1.6		Amplitude not reduced.		

Table C.

Description.	Energy loss and electric intensity relation.	Per cent formula.	Change with speed variation.
Selenium:			
Commercial A	Immeasurably large loss.		
Commercial B	Index 1.96	Similar to next, but 3.4 times larger at 5000 volts.	Nil.
Commercial C	Index 1.96	Amplitude about same as next, *D*.	Small increase.
Pure D	$W = 4.6\,F^{1.85}$	More than 400 per cent.	No observations.
Resin: Many spheres.	Index varied from 2.8 to 1.86. Loss $F = 1$ from 4 to 18 per cent.		Small increase?
	Rapid field rotating between 10^6 and 10^7 per second. Ebonite, glass, and complex sulphur gave no loss at all, even though the electric intensity employed was considerably greater than in the other experiments here summarized.		
	Heterogeneous conductors. Paraffin and resin, mixed with large quantities of graphite, gave results not much greater than when the pure dielectrics were employed. The index in these cases was about 2.		
	Conductors: aluminium, paraffin coated with tin-foil, arc light carbon. Resin and paraffin, made conducting with graphite, gave no effect whatever in slow fields.		
	Bisulphide of carbon, benzene, and kerosene were not set in rotation by the rotating field.		

It results from the above investigation (1) that the effect of moisture is very far from being eliminated by the Arno method, though the circumstances are very favorable in the sense that moisture should have a smaller effect than in experiments involving rapid space variations of the electric forces.

Nevertheless, prolonged drying will bring the insulators examined down to a steady state, and a residual hysteresis effect makes its appearance.

This effect can be represented over a range of potential differences of from 0 to 6000 volts with an accuracy of the order of the accuracy of observation by an expression of the form $W = aF^n$. No physical conclusion, however, can be drawn from such a formula, for it merely sums up the results of many complex processes; and it is notorious that a formula of the kind quoted can be made to fit almost any curve over a short range. The values of n vary from about 1.5 to about 1.95 for ordinary "homogeneous" dielectrics. When dielectrics are made heterogeneous by the addition of graphite, the index rises to 2 and over, and the effects become irregular. This result was not unforeseen, for I had previously found that sulphur conducts discontinuously, and conduction in heterogeneous dielectrics is probably an important feature of the phenomenon. The rise of the index above 2 is to be explained on Hess's theory by the remark that the conduction does not follow Ohm's law; and in terms of the experiments by the remark that if the rate of subsidence of the polarization of the dielectric due to conduction is comparable with the period of field rotation, the energy put into the dielectric during one rotation is quite indeterminate, and the drag depends on the angle between the intensity and the polarization, which in turn depends on the non-ohmic conductivity.

(2) The hysteresis so discovered is quite constant for any given specimen, but is very variable as we pass from specimen to specimen, the index of the empirical expression of the variation being much more constant than the coefficient. It is known that very small differences in physical condition produce immense variations in the hysteresis loss of iron.

The hysteresis of dielectrics resembles the magnetic hysteresis of iron in this respect, but the phenomenon is on the whole still more strongly marked.

(3) The great differences between the values of specific inductive capacity assigned to certain substances by different observers, as well as the results here obtained, suggest that perhaps it is nearly as futile to treat the specific inductive electric capacity of a "substance" as distinguished from a "specimen" as constant, as to extend the same treatment to the specific inductive magnetic capacity of iron, even under given magnetic forces.

(4) The mechanics of the problem of the rotating field require that there should be an angular interval between the direction of the electric polarization and the direction of the electric force, in order that there may be any drag on the suspended spheroid. The problem is essentially similar to the problem of the couple exerted by the moon on the earth about the axis of the latter, and due to the want of correspondence between the phases of the tide and of the tide-raising force.

The fact that there is a drag on the spheroid exerted by the rotating field implies either (1) that dielectric polarization takes an appreciable time to become established, or (2) that dielectric polarization does not die away in simple proportion as the force is reduced. The present experiments do not enable us to discriminate between the phenomena, and it is most probable indeed that one implies the other. The experiments of observers on the variation of specific inductive capacity, expressed as a function of the period of charge, lead to the same result, but I believe that they have all been affected to some extent by surface conduction.

(4) When I commenced these experiments I hoped to increase the angle between the direction of polarization and the direction of electric force by increasing the rate of rotation of the electric field. Assuming for a moment that the time required for the establishment of the polarization is merely the time required for the inwards sideways motion of the Faraday tubes, we see that for experiments of the frequency adopted in the mechanically rotating cell, the polarization may be considered as established instantaneously. In this case, the drag is due to the persistence of the polarization, and would become a maximum when the rotation was so fast as to make the angle between the electric force and the polarization vectors exactly 90°. A reference to the summary will make it clear that when the dielectrics were dry the drag was either inappreciably affected by increasing the rate of rotation or was increased thereby. At a frequency about a million the effect vanished. There is clearly a speed of maximum effect somewhere between a frequency of rotation of 30 and of a million per second. If I had had a high frequency alternator at my disposal, I should certainly have attempted to run this down, for I think that the measurement of the period of persistence which would be deduced could not fail to give most important information.

(5) The Maxwell suggestion as to the cause of residual discharge does not, I think, account for the results of these experiments. My former experiments on sulphur (*loc. cit.*) show most unmistakably that the breakdown of polarization is of an electro-chemical nature, and this fact is sufficient to account for residual charge if the ions are not free to reach the electrodes. The present series of experiments indicates how the time required for this break-down may be measured by any one fortunate enough to possess a couple of alternators of high and low frequency, respectively.

In conclusion, I desire again to thank those who have assisted me, — Mr. Alfred Pepper, in the earlier experimental work, Professor Lyle, who supplied the substance of the mathematical investigation, and Mr. Pollock, who enabled me to apply the same, and who kindly checked a typical set of observations and reductions.

University of Sydney, New South Wales,
Nov. 23, 1896.

A DETERMINATION OF THE SPECIFIC RESISTANCE AND TEMPERATURE COEFFICIENT OF OIL IN THIN FILMS, AND THE APPLICATION OF THESE RESULTS TO THE MEASUREMENT OF THE THICKNESS OF THE OIL FILM IN JOURNAL BOXES.

By F. L. O. Wadsworth.

Introductory Note.

THE results presented in the following paper were obtained several years ago while I was a student at the Ohio State University. The second part of the investigation had just been begun when the investigation was interrupted by my departure from the University. Since then I have always hoped to be able to carry the work to completion, but the continued pressure of other duties has so far prevented me from doing this. As I am now permanently engaged upon other entirely different lines of investigation, I have decided to publish the account of the work already completed in the hope that the results so far obtained may not be uninteresting in themselves.

The question of how to determine the thickness of oil in a journal box under actual conditions of work, and find how it varies with varying conditions of speed and pressure, is a most interesting and important one to the mechanical engineer; but it has not yet, so far as I am aware, been satisfactorily answered. The method proposed at the end of the paper seems a promising one, to judge from the few preliminary experiments which were made; and I hope some one may be sufficiently interested in the problem to continue the work along this line.

Considering the length of time that has now elapsed since the article describing the results of the measurement of the resistance of films was written, it has been thought best to leave it in the original form except for a few minor changes in wording or arrangement. Footnotes have been added wherever it was thought desirable to make modifications or corrections.

Yerkes Observatory, University of Chicago.
March, 1897.

Very little work seems to have been done in determining the specific resistance and temperature coefficient of the various kinds of oil. The only experiments which the writer has been able to find are those of Brooks[1] on the *relative* resistances of a sample of paraffine oil for various temperatures between 4.4 C. and 93.3 C.[2] The experiments were made on a sheet of oil 3 mm. thick and about 2.3 sq. cm. in cross section. No data, however, were given from which the absolute resistance could be calculated, nor was it stated what particular kind of paraffine oil was used.

In the experiments recorded below, which have been undertaken as preliminary to the determination of the thickness of oil in journal bearings, the oil used was the best quality of commercial sperm oil, in which zinc had been placed for over a year, in order to free it from any acid which it might contain. In addition to the determination of the absolute specific resistance and temperature coefficient, another object of these experiments was to determine whether there was any variation of specific resistance with variation in the thickness of the film,[3] or with the strength of current used.

Apparatus and Method.

In order to obtain a thin uniform film of oil of a determinate thickness, the following arrangement was used :

A pair of fine (Brown & Sharpe) surface plates, size about 4 by 6 inches, were placed one above the other with their surfaces separated a small distance by pieces of thin sheet rubber or parafined paper.[4] At first the capillary attraction between the plates was relied upon to keep the film of oil uniformly spread between the two adjacent surfaces, but this being found sometimes unreliable, the two plates were immersed in a sheet iron tank filled with the oil under examination to a height just above the surface of the under plate (See Fig. 1). In order to avoid any danger of air-bubbles in the film of oil between the plates, the upper plate was removed before beginning each series of experiments, and the oil heated to a temperature of about 50°. The pieces of insulating material were then placed in position, and the upper plate let down upon the lower one, care being taken to make the edges of the two plates exactly coincide. To drive out the bubbles of air which might cling to the surface of the upper plate when it was let down, the two plates were brought together with a quick hinge-like motion. The plates and the tank of oil were then allowed to cool to the temperature of the room before beginning the measurements. The temperature was observed by means of two thermometers, one immersed in the oil of the tank, the other immersed in oil contained in the cavity of the upper surface plate. The resistance of the film of oil between the two plates was measured by comparing it with a 250,000 ohm box, by the method of deflections. The arrangement of the circuit is shown in Fig. 1, where

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[1] Wiedmann's Electricitat, Vol. II., p. 5.

[2] Since these experiments were made a considerable amount of work has been done in determining the insulation resistance of various kinds of oil, particularly for the very high electromotive forces used in alternate current transmissions.

[3] I did not learn of Reinhold and Rucker's results on the electrical resistance of thin soap films until some time after this work had been completed. (See also subsequent foot note on p. 93.)

[4] A method very similar to this in principle has since been used by Prof. Threlfall for obtaining the resistance of a thin film of sulphur. Phil. Mag. Vol. 38, p. 452. 1889.

G is a Thomson galvanometer of 6,000 ohms resistance.

R is a 250,000 ohm box (of platanoid correct at 18° C., temp. coefficient +.0002.)

T is a tank containing the two surface plates, t_1 and t_2, arranged as before described.

K is a three way key for changing from the circuit through *R* to the circuit through t_1 and t_2.

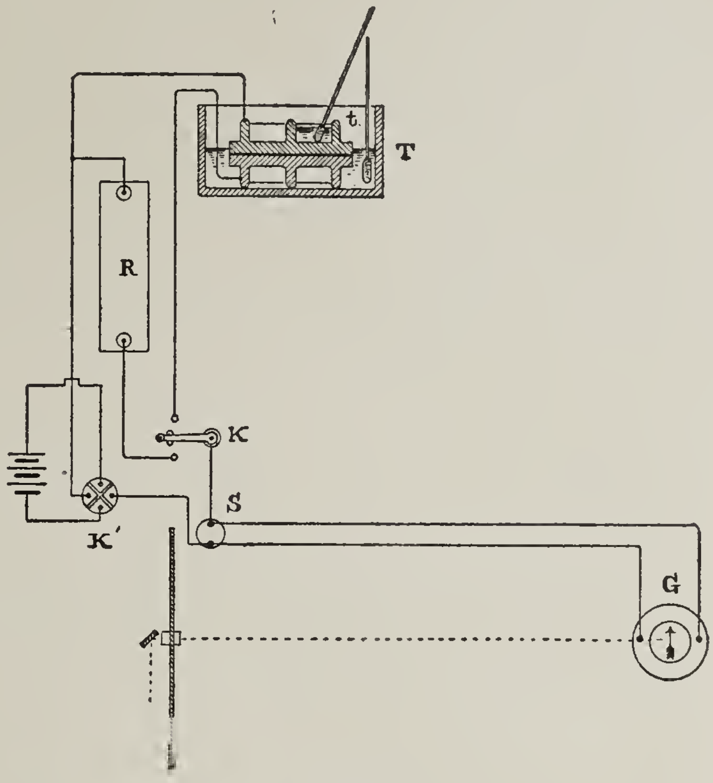

Fig. 1.

K' is a reverse key for reversing the current in the battery circuit.

S is a shunt in the galvanometer circuit.

Let G be the resistance of the galvanometer; R the resistance of the box; Q the resistance of the oil film; B the resistance of the battery and rest of the circuit (which may be neglected in comparison with either Q or R); S_1 and S_2 the shunts used respectively with R and Q; d_1 the deflection of the galvanometer when R is in circuit and d_2 the deflection when Q is in circuit. Then if d_1 and d_2 are not greatly different, it has been proven

$$\frac{R+G+B}{Q+G+B}=\frac{d_2}{d_1}\cdot\frac{\frac{G+S_2}{S_2}}{\frac{G+S_1}{S_1}} \qquad (1)$$

or, since G is always very small in comparison with Q, and B may be neglected, we have

$$Q=(R+G)\frac{d_1}{d_2}\cdot\frac{S_2}{S_1}\cdot\frac{G+S_1}{G+S_2} \qquad (2)$$

The resistance Q is made up of two factors; one the resistance of the oil film itself; the other the resistance of the insulating pieces which are used to separate the two plates. It has been found by direct experiment that the resistance of these pieces is so high that the measured resistance Q may be considered as produced by the oil film alone.[1]

In order to determine whether variation in the current strength produced any variation in the resistance of the oil film, each film was tested with four currents, *i. e.*:

1st. That produced by one Lelanche element, E. M. F. about 1.4 volts.

2d. Two Lelanche elements in series, E. M. F. about 2.8 volts.

3d. Three bichromate elements in series, E. M. F. about 5.5 volts.

4th. Twelve cells of storage battery in series, E. M. F. about 25 volts.

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[1] The specific resistance of sheet rubber is nearly half a million times greater than that found for the oil under test, and the resistance of paraffine is even higher.

The results are given in the tables which follow. As far as could be determined, there was no systematic difference corresponding to the differences in electromotive forces in the circuit.

When the thickness of the oil film was less than 0.″01 some trouble was experienced from a gradual change in resistance, seemingly due to polarization of the oil film. Thus with a film 0.″01 in thickness with three bichromate cells in series and a 1-10 shunt in the galvanometer circuit, successive readings at intervals of one minute gave :

deflection right—368, 367.5, 367, 366.5, 365, 364.5, 363.5, 363, etc.

deflection left (battery reversed)—135, 136.5 ; after considerable interval—137.

To prove that this was not due to any change in the battery current *R* was brought into circuit. The deflection both right and left remained perfectly steady for many minutes.

As the fall was very slow (only ½ div. or less than ½ per cent. of the whole deflection in a minute), only a very small error will be committed if the deflection is taken as soon as the oscillation of the needle due to closing the key *K* (which, as the needle is strongly damped, takes place at the end of the second or third swing, or in from 20 to 30 seconds), has ceased. It is hardly necessary to add that in determining d_1 and d_2 the usual precaution of reversing the battery current at each observation and taking the mean of the two successive deflections (right and left) was always adopted. At frequent intervals the galvanometer circuit was also tested by reversal to be sure that there were no errors introduced by thermal effects.

In the case ultimately under consideration (that of measuring the resistance of a film of oil between the moving surface of a shaft and the journal box), polarization would of course be prevented by a constant movement of the layer of oil.

Determination of Specific Resistance.

In order to obtain the specific resistance from the measurements of *Q* we must also know :

1. The area of the conducting film. This will be of course, the area of the plates, less the area of the supporting pieces, which was always 0.5 sq. in.

2. The thickness of the film. The thickness of the film was determined by the measurement of the thickness of the separating pieces. These pieces were measured very carefully by a Betts measuring machine reading to 0.″0001. This measurement, however, might not always give the true average thickness of the film of oil for the following reasons :

a. Because of the compression of the pieces when in use by the weight of the upper surface plate, a compression which is increased by the preliminary heating and subsequent contraction of the oil and plates, the contraction of the oil film drawing the plates together and compressing the separating pieces in some instances by as much as .001 of an inch, as determined by measurement before and after using. To avoid this error the measurements taken were always those which were made after the pieces had been used.

b. Because of inequalities in the thickness of different pieces. The error due to this cause is very small, as care was always taken to select pieces which were originally of as nearly the same thickness as possible. The differences found on the measuring machine never amounted to more than two per cent., and were generally much less. Hence the average of the pieces could be taken as representing the average thickness of the oil film.

c. Because of the deflection of the plates between the points of support. This deflection, although exceedingly small, cannot be considered as negligible when compared to the thickness of the very thinnest of these films, *i. e.*, about 0.″004. To prevent this effect as far as possible, the supporting pieces are so placed that their distance from the center lines of the plates was $l/2\sqrt{3}$, where l is the distance across the plate at right angles to the centre lines.

d. Because of inequalities in the surfaces themselves. Though the two surfaces are in general accurately plane, nevertheless when considered in detail they consist of a number of points which can readily be seen and distinguished by rubbing the plates together for a short time, until the points of contact become bright and polished. Between these points are shallow cavities due to the scraping. The error due to this cause is of contrary sign to those *a* and *c*, and its effect will be particularly considered later.

If A denotes the effective area of the oil film, a its thickness, and

Q_t the measured resistance at the temperature t, we have for the specific resistance (C. G. S. units) σ_t at this temperature, if A and a are expressed in inches,

$$\sigma_t = 2.54\ Q_t \frac{A}{a} \tag{3}$$

For the specific resistance at 0° or at any standard temperature t_0, we have

$$\sigma_0 = 2.54\ Q_t \frac{A}{a} f(t-t_0) \tag{4}$$

where $f(t-t_0)$ is the temperature coefficient of the oil.

To determine this function the oil in the tank was heated to an initial temperature of about 115° C., and then, after the whole had begun to cool uniformly a series of simultaneous readings of temperature and resistance were taken while the oil and plates were cooling. As before, two thermometers were always used; one immersed in the oil tank and the other placed in a cavity filled with water in the upper surface plate. The oil was kept thoroughly stirred, and the mean of the two readings which (except for the higher temperatures while the plates were cooling rapidly) differed by only a slight amount, taken as representing the temperature of the oil film. For convenience these measurements were made on the thinnest oil film used (thickness about 0.″005), but there is no reason to suppose that the results would have been different for any of the other films.

Dimensions of Apparatus.

The size of the surface plates as measured by a Brown & Sharpe Vernier caliper was 4.45 by 5.95 inches. The total area of the plates was therefore 26.5 sq. in. The insulating pieces were always cut of such size that their combined area was as already stated 0.5 sq. in. The area of the oil film was therefore 26 sq. in. The films tested were of four different thicknesses, as follows:

Series A.—The insulating material was sheet rubber. Three pieces were used, of the following thicknesses:

No. 1. Average of six measurements gave . . . 0.″01822
No. 2. " " " " " . . . 0.″01790
No. 3. " " " " " . . . 0.″01827
Average thickness of three pieces (=average thickness of oil film) . . . . 0.″01813

Series B.—The insulating material was sheet rubber. Four pieces were used of the following thicknesses:

No. 1. Mean of six settings gave	0.″0601
No. 2. " " " " "	0.″0613
No. 3. " " " " "	0.″0602
No. 4. " " " " "	0.″0615
Average thickness of pieces (=average thickness of film)	0.″0608

Series C.—The insulating material was paraffined paper. Four pieces were used of the following thicknesses:

No. 1. Mean of six settings gave	0.″01019
No. 2. " " " " "	0.″01017
No. 3. " " " " "	0.″01030
No. 4. " " " " "	0.″01017
Average thickness of pieces (=average thickness of oil film)	0.″01021

Series D.—The insulating material was paraffined paper. Four pieces were used, but unfortunately they were accidentally destroyed before they were measured on the Betts measuring machine. They had been measured by micrometer caliper before being used, and the thickness so obtained, which was 0.″0075, is probably correct within two or three per cent.

Series E.—*Combined observations for specific resistance and temperature coefficient.* The material used in this case was paraffined paper. Four pieces were used of the following thicknesses:

No. 1. Mean of six settings gave	0.″0046
No. 2. " " " " "	0.″00463
No. 3. " " " " "	0.″00467
No. 4. " " " " "	0.″00461
Average thickness of pieces (=average thickness of oil film)	0.″00462

The results of the measurements with these different thicknesses and with the varying strength of battery current are given in the following tables:

TABLE I. (SERIES A.)

Battery.	Remarks.	Shunt. $\frac{S_1+G}{S_1}$	Shunt. $\frac{S_2+G}{S_2}$	Mean d_1	Mean d_2	Q_t from (2).	t*	Q_0 from (7).	δ
						megohms		megohms	
1 Leclanche cell. . . .	. . . .	$\frac{1}{100}$	1	28.75	61.5	11.97	16° C.	10.53	—.10
" " . . .	Plates separated and pieces rearranged.	$\frac{1}{100}$	1	29.25	62.75	11.93	16	10.49	—.14
" " . . .		$\frac{1}{100}$	1	29.40	64.50	11.67	17	10.58	—.05
" " . . .	Pieces rearranged again.	$\frac{1}{100}$	1	29.50	65.25	11.59	17	10.51	—.12
" " . . .		$\frac{1}{100}$	1	29.70	66.25	11.48	17.5	10.57	—.06
" " . . .	. . . .	$\frac{1}{100}$	1	29.50	66.25	11.40	18.0	10.66	+.03
2 " " (series)	. . . .	$\frac{1}{100}$	$\frac{1}{10}$	91.00	20.65	11.28	"	10.55	—.08
" " "	. . . .	$\frac{1}{100}$	$\frac{1}{10}$	91.75	21.00	11.19	"	10.47	—.16
3 Bich. " "	. . . .	$\frac{1}{1000}$	$\frac{1}{10}$	28.85	67.35	10.97	20	10.97	+.34
" " "	. . . .	$\frac{1}{1000}$	$\frac{1}{10}$	28.75	67.40	10.92	20.5	11.11	+.48
Storage (12 cells). . .	. . . .	$\frac{1}{1000}$	$\frac{1}{100}$	98.75	22.50	11.23	18	10.52	—.11
" " . .	Pieces rearranged again.	$\frac{1}{1000}$	$\frac{1}{100}$	98.50	22.50	11.21	"	10.51	—.12
" " . .		$\frac{1}{1000}$	$\frac{1}{100}$	98.45	22.10	11.41	18+	10.68	+.05
1 Leclanche cell. . . .	Taken 24 hrs. later after completely rearranging apparatus.	$\frac{1}{100}$	1	36.00	83.35	11.06	19	10.69	+.06
" " . . .		$\frac{1}{100}$	1	36.50	84.90	11.01	"	10.65	+.02

Mean resistance Q_0 at 20° C. 10.63+
Probable error of mean (15 observations) ±.030

* Temperatures were not observed as closely as they should have been this first day.

TABLE II. (SERIES B.)

Battery.	**Remarks.**	**Shunt.** $\frac{S_1+G}{S_1}$	**Shunt.** $\frac{S_2+G}{S_2}$	**Mean** d_1	**Mean** d_2	Q_t from (2).	t	Q_0 from (7)	δ
						megohms		megohms	
Storage battery.		$\frac{1}{1000}$	$\frac{1}{10}$	97.75	75.75	33.03	18°+C.	30.90	—
“ “		$\frac{1}{1000}$	$\frac{1}{100}$	98.35	7.65	32.92	18.5	31.32	—
“ “	After standing some time undisturbed.	$\frac{1}{1000}$	$\frac{1}{10}$	96.30	69.50	35.48	18.5	33.76	+.125
“ “	Pressure on top plate.	$\frac{1}{1000}$	$\frac{1}{10}$	97.60	71.75	35.00	18.7	33.51	−.125
“ “	Pressure removed.	$\frac{1}{1000}$	$\frac{1}{10}$	96.25	69.85	35.28	“	33.77	+.135
“ “	Pieces rearranged.	$\frac{1}{1000}$	$\frac{1}{10}$	96.85	70.25	35.29	“	33.77	+.135
3 Bich. cells (series).		$\frac{1}{1000}$	$\frac{1}{10}$	28.25	20.75	34.84	18.8	33.47	−.165
“ “ “		$\frac{1}{1000}$	$\frac{1}{10}$	28.45	20.85	34.92	“	33.55	−.085
2 Leclanche cells (series)		$\frac{1}{100}$	1	97.35	71.75	34.74	19	33.59	−.045
“ “ “		$\frac{1}{100}$	1	97.50	72.40	34.48	“	33.35	−.285
1 Leclanche cell.		$\frac{1}{100}$	1	36.80	27.45	34.33	19.5	33.75	+.115
“ “	Places separated and pieces rearranged.	$\frac{1}{100}$	1	36.90	27.50	34.36	“	33.77	+.135
“ “		$\frac{1}{100}$	1	36.90	27.55	34.28	“	33.70	+.065

Mean resistance Q_0 at 20° C. 33.635

Probable error of mean (11 observations) ±.030

Table III. (Series C.)

Battery.	Remarks.	Shunt. $\frac{S_1+G}{S_1}$	Shunt. $\frac{S_2+G}{S_2}$	Mean d_1	Mean d_2	Q_t from (2).	t	Q_0 from (7).	δ
						megohms		megohms	
3 Bich. cells (series)	Only three pieces used . .	$\frac{1}{1000}$	$\frac{1}{10}$	28.5	111.05	6.57	22° C.	7.06	+.20
" " "	. . . .	$\frac{1}{1000}$	$\frac{1}{10}$	28.35	111.75	6.50	22	6.98	+.12
" " "	Pieces readjusted end for end.	$\frac{1}{1000}$	$\frac{1}{10}$	28.45	115.50	6.31	22	6.78	−.08
2 Leclanche cells "	. . . .	$\frac{1}{100}$	$\frac{1}{10}$	99.70	40.50	6.31	22	6.78	−.08
1 " cell. . . .	. . . .	$\frac{1}{100}$	1	39.75	161.50	6.31	22.5	6.90	+.04
" " . . .	Fourth piece placed under plate.	$\frac{1}{100}$	1	39.75	166.00	6.12	23.0	6.82	−.04
" " . . .	Pressure applied to top plate.	$\frac{1}{100}$	$\frac{1}{10}$	39.75	16.85	6.04	23.0	6.73	−.13
" " . . .	. . . .	$\frac{1}{100}$	$\frac{1}{10}$	39.75	17.45	5.84	23.5	6.64	−.22
" " . . .	After standing 12 hrs.	$\frac{1}{100}$	$\frac{1}{10}$	43.15	18.00	6.14	22.0	6.60	−.26
" " . . .	Pieces rearranged.	$\frac{1}{100}$	$\frac{1}{10}$	43.40	18.00	6.17	22.5	6.75	−.11
" " . . .	. . . .	$\frac{1}{100}$	$\frac{1}{10}$	43.40	18.05	6.17	22.5	6.75	−.11
" " . . .	After standing 6 hrs.	$\frac{1}{100}$	$\frac{1}{10}$	44.50	14.80	7.70	17.8	7.16	+.30
" " . . .	Pieces rearranged.	$\frac{1}{100}$	$\frac{1}{10}$	44.50	14.85	7.68	17.8	7.14	+.28
" " . . .	. . . .	$\frac{1}{100}$	1	44.00	150.00	7.51	17.9	7.00	+.14
" " . . .	Heated to temp. about 40° and cooled.	$\frac{1}{100}$	$\frac{1}{10}$	43.70	22.25	5.02	25.0(?)	——(?)	——

Mean resistance Q_0 at 20° C.	6.86+
Probable error of mean (14 observations)	±.032

Table IV. (Series D.)

Battery.	Remarks.	Shunt. $\frac{S_1+G}{S_1}$	Shunt. $\frac{S_2+G}{S_2}$	Mean d_1	Mean d_2	Q_t from (2)	t	Q_0 from (7)	δ
						megohms		megohms	
3 Bich. cells (series)	Only 3 pieces used.	$\frac{1}{1000}$	$\frac{1}{10}$	27.80	154.00	4.62	20° C.	4.62	—.36
" " "	Plates separated and pieces readjusted.	$\frac{1}{1000}$	$\frac{1}{10}$	27.75	148.50	4.78	19.9	4.76	—.22
" " "	Pieces readjusted and fourth piece added.	$\frac{1}{1000}$	$\frac{1}{100}$	27.75	146.00	4.86	19.8	4.82	—.16
" " "	. . . .	$\frac{1}{1000}$	$\frac{1}{10}$	27.75	143.50	4.95	"	4.91	—.07
" " "	After standing some time.	$\frac{1}{1000}$	$\frac{1}{10}$	27.60	138.50	5.10	19.5	5.01	+.03
" " "	Pieces readjusted.	$\frac{1}{1000}$	$\frac{1}{10}$	27.50	140.75	5.00	"	4.92	—.06
" " "	After standing 12 hours undisturbed.	$\frac{1}{1000}$	$\frac{1}{10}$	28.05	117.50	6.00	15.8	5.20	+.22
" " "	Pieces readjusted.	$\frac{1}{1000}$	$\frac{1}{10}$	28.15	121.50	5.93	16.2	5.21	+.23
2 Léclanche "	. . . .	$\frac{1}{100}$	$\frac{1}{10}$	103.50	48.75	5.44	17.0	4.93	—.05
" "	Pieces readjusted.	$\frac{1}{100}$	$\frac{1}{10}$	106.00	49.25	5.51	17.1	5.00	+.02
1 Léclanche cell	. . . .	$\frac{1}{100}$	$\frac{1}{10}$	45.65	20.90	5.59	"	5.08	+.10
" "	. . . .	$\frac{1}{100}$	$\frac{1}{10}$	45.65	21.00	5.57	17.4	5.10	+.12
' "	One piece taken out and others readjusted.	$\frac{1}{100}$	$\frac{1}{10}$	45.75	21.25	5.51	"	5.05	+.07
" "	Pieces readjusted again.	$\frac{1}{100}$	$\frac{1}{10}$	45.85	21.75	5.40	17.6	4.99	+.01
" "	Pressure applied.	$\frac{1}{100}$	$\frac{1}{10}$	46.10	21.75	5.48	"	5.06	+.08
" '	" removed.	$\frac{1}{100}$	$\frac{1}{10}$	45.50	21.50	5.42	17.8	5.04	+.06

Mean resistance Q_0 at 20° C	4.98	
Probable error of mean (16 observations)		±.026

Table V.[1] (Series E.)

Shunt. $\frac{S_1+G}{S_1}$	Shunt. $\frac{S_2+G}{G}$	Mean d_1	Mean d_2	Q_t from (2)	t mean.	Q_0 from (7)
				megohms		megohms
$\frac{1}{100}$	$\frac{1}{100}$	45.85	35.85	0.327	88°5 C.	
“	$\frac{1}{100}$	45.35	29.25	0.397	77.5	
“	$\frac{1}{100}$	45.75	25.10	0.467	73.25	
“	$\frac{1}{100}$	45.75	23.60	0.496	71.0	
“	$\frac{1}{100}$	45.75	21.25	0.551	67.0	
“	$\frac{1}{100}$	45.75	17.60	0.666	61.25	
“	$\frac{1}{100}$	45.70	16.45	0.711	59	
“	$\frac{1}{100}$	45.70	13.75	0.851	55	
“	$\frac{1}{100}$	45.70	12.95	0.903	53.25	
“	$\frac{1}{100}$	45.70	9.55	1.225	42.5	
“	$\frac{1}{10}$	45.70	93.50	1.251	42.0	
“	$\frac{1}{10}$	45.80	85.50	1.371	40.9	
“	$\frac{1}{10}$	45.80	71.25	1.646	35.9	
“	$\frac{1}{10}$	45.80	68.50	1.712	34.85	
“	$\frac{1}{10}$	45.85	65.00	1.806	34.60	
“	$\frac{1}{10}$	45.85	62.25	1.886	33.40	
“	$\frac{1}{10}$	45.85	53.00	2.215	29.20	
“	$\frac{1}{10}$	45.90	49.75	2.362	27.80	
“	$\frac{1}{10}$	45.95	46.50	2.530	26.45	
“	$\frac{1}{10}$	45.95	44.25	2.66	25.00	3.21
“	$\frac{1}{10}$	45.95	42.25	2.76	24.00	3.21
“	$\frac{1}{10}$	45.95	41.75	2.82	23.50	3.20
“	$\frac{1}{10}$	45.10	33.05	3.49	16.8	3.14
“	$\frac{1}{10}$	45.10	33.00	3.50	16.8	3.15

Mean resistance Q_0 at 20° C. 3.18+

In order to reduce the observed resistance at different temperatures to a standard temperature it is necessary to first determine the temperature coefficient from the observations of Series E. For this purpose these observations were first plotted (Fig. 2) and by the aid of a flexible steel strip a smooth curve was drawn, which represented most closely the experimental results. To represent this curve empirically an equation of the usual series form, *i. e.*,

$$Q_t = Q_0\left[1 + A_1(t-t_0) + A_2(t-t_0)^2 + A_3(t-t_0)^3\right] \qquad (5)$$

---

[1] One Leclanche cell used throughout. Last two observations made 48 hours later, after plates had been separated and put together again.

was chosen, three terms being judged sufficient to represent the results over the range of temperature observed (90° to 15° C). In order to determine the values of Q_0, A_1, A_2, and A_3, the theoretically best method is to form a series of observation equations by substituting in (5) the observed values of Q_t and t, and deduce from them four normal equations by the method of Least Squares. But in

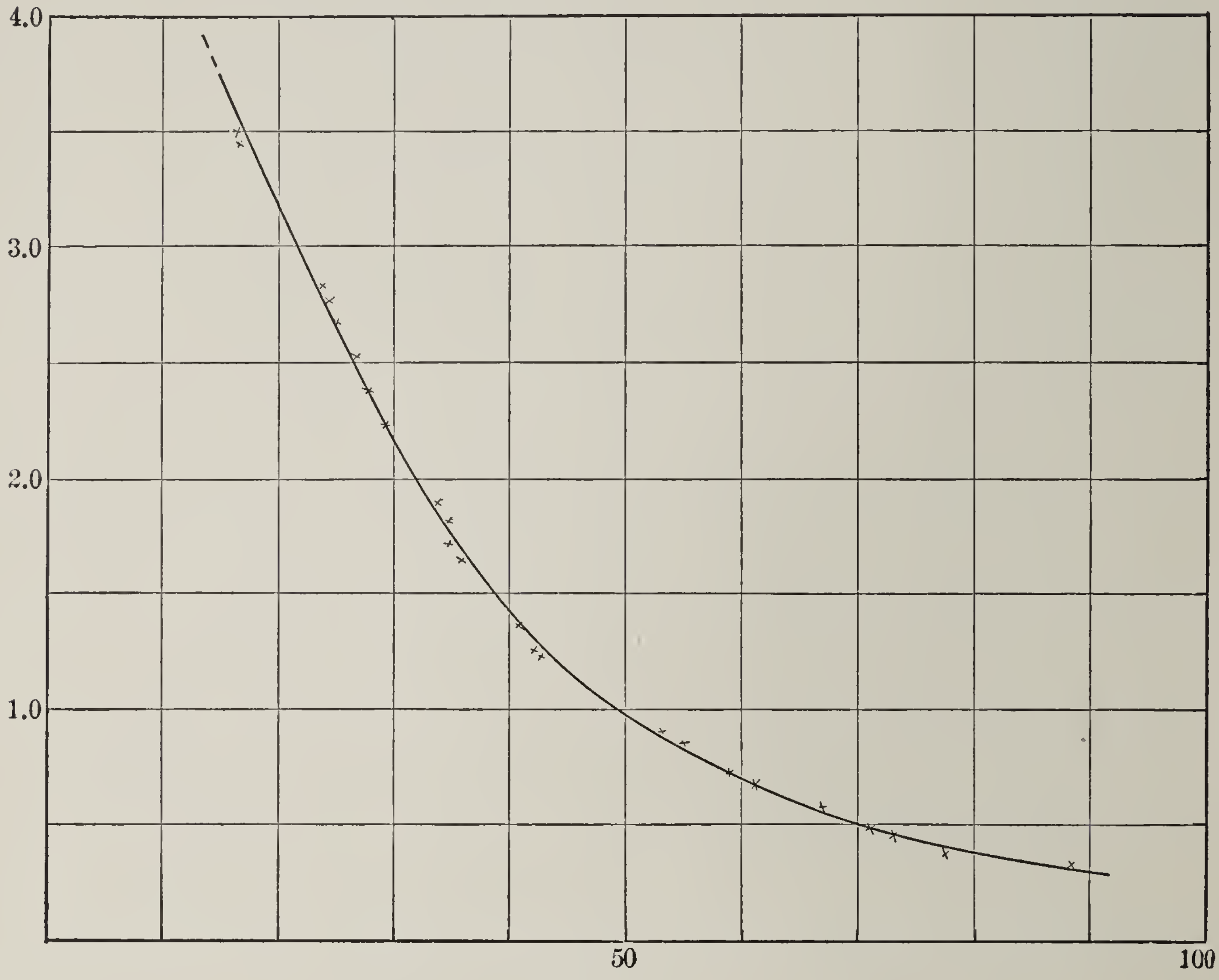

Fig. 2.

this case the observed points fell so nearly on the smooth trial curve that it was judged sufficient to determine the values of these constants directly from four carefully selected points on the trial curve. The points so selected were those corresponding to the four temperatures of 20°, 30°, 50°, and 80°. The temperature selected as standard for these experiments was 20°, as being about the mean temperature of all the series taken, and as therefore involving the least chance of error in the reductions of the observations to this temperature. The value of Q_0 is therefore at once determined. From inspection of the trial curve it was found to be

$$Q_{20^\circ}=3.18 \text{ megohms.}$$

This value agrees almost exactly with that afterward found in reducing Series E.

For other points we find

$$Q_{30^\circ}=3.18\,(1+10\,A_1+100\,A_2+1000\,A_3)=2.15$$
$$Q_{50^\circ}=3.18\,(1+30\,A_1+900\,A_2+27000\,A_3)=.983$$
$$Q_{80^\circ}=3.18\,(1+60\,A_1+3600\,A_2+216000\,A_3)=.370$$

The values of A_1, A_2 and A_3, determined from these equations, are respectively

$$A_1=-0.038$$
$$A_2=+0.00061$$
$$A_3=-0.0000037$$

Equation (5) thus becomes

$$Q_t=3.18\left\{1-0.038\,(t-20^\circ)+0.00061\,(t-20^\circ)^2-0.0000037\,(t-20)^3\right\} \tag{6}$$

The curve represented by this equation between $t=15^\circ$ and $t=100^\circ$ is plotted in Fig. 2. By inspection it will be seen that it represents the results of experiment very well; probably almost if not quite as well as it would had the values A_1, A_2, and Q_0, all been determined by the theoretically more precise, but far more laborious process of Least Squares. Indeed, in this case the amount of labor involved (as in many others where the Least Square process is commonly employed), would have been vastly disproportionate to the results obtained, and was, therefore, unwarranted by the accuracy of the observations themselves.

Between the points $t=15^\circ$ and $t=25^\circ$ (between which all the observations of the first four series lie) the curve corresponds almost exactly to a straight line, and can therefore be represented by a simpler expression of the form.

$$Q_t=Q_0\,(1-\beta\,t) \tag{7}$$

The mean value of β between these limits, as determined from the tangent to the curve at the point $t=20^\circ$, is

$$\beta=0.0343.$$

The temperature coefficient of the oil is represented by the expression in the parenthesis of (6), or

$$f(t-20^\circ)=1-0.038\,(t-20^\circ)+0.00061\,(t-20^\circ)^2-0.0000037\,(t-20^\circ)^3 \qquad (8)$$

or between 15° and 25° by the simpler expression from (7)

$$f(t-20^\circ)=1-0.0343\,(t-20^\circ). \qquad (9)$$

By the aid of (9) the observations in the five tables have been reduced to the chosen standard temperature of 20°, and the means, taken as representing the mean resistance of the five films at 20° C. As before stated, three and in some cases four sets of measurements were made in each series, with different strengths of battery current. It will also be noted from the remarks that the plates were separated and the insulating pieces readjusted several times during each set of measurements, that the conditions of experiment were varied in many other ways (such as applying and then removing pressure from the top plate, heating and cooling the plates, when in position, varying the number of supporting pieces between the plates, etc.), and that each series extended over two or three days. Under these circumstances, and considering also the number of observations, it may be fairly considered, I think, that in the mean the accidental errors of experiment have been eliminated. For the same reasons the probable errors deduced from each set may be taken as fairly representing the accuracy of the results attained. None of the observations have been omitted save the first two in Series B, which were obviously considerably in error (probably sufficient time was not given the plates to cool down after the first setting, as it will be noticed that the resistance rose to a value very near the mean "after standing for some time undisturbed;") and one in Series C, where the temperature was uncertain, having been taken too soon after heating. There are several that might have been rejected on Chauvenet's criterion and the apparent accuracy thereby considerably increased, but such as arbitrary treatment of the results of physical measurement on purely mathematical grounds, and where there has been no apparent cause of suspicion registered at the time of making these particular observations, has always seemed to me a very dangerous and unwarranted proceeding.[1]

An inspection of these probable errors (see Table VI.) shows, as might have been expected, that the accuracy of the determination decreases with the thickness of the film of oil. The error in the case of the thick film (Series B) is less than one-tenth of one per cent., while the largest error in the case of the two thin films is only half of one per cent., a degree of accuracy considerably greater than is necessary for the purpose for which these observations were originally made.

Table VI.

Series.	Mean resistance 20°.	Error in per cent.	Thickness.	Specific resistance at 20°; megohms per cu. cm.
B	33.635 ± 0.030	0.0009	0″.0608	36500
A	10.63 ± 0.030	0.0025	0.01813	38700
C	6.86 ± 0.032	0.0046	0.01021	44300
D	4.98 ± 0.026	0.0051	0.0075	43800
E	3.18	about 0.005 [2]	0.00462	45400

When we calculate the specific resistance from these results we find an interesting result; viz., that the specific resistance increases regularly as the thickness of the film decreases. This is shown in Table VI., and better in Figs. 3 and 4. In Fig. 3 the means of the different series are plotted as a function of the thickness, *a*, and in the second the absolute specific resistances calculated from these first results are plotted in the same way. With the exception of the one result from Series C, the points all lie on a smooth curve, which in the first case passes through the origin, and in the second appears to be asymptotic to the two axis of σ and a. The variation is of too regular a character to be accidental. I had indeed anticipated that such a result might be obtained before beginning the experimental work, and it was for that reason that I chose to work on films as thin as I could conveniently obtain rather than on the oil in mass.

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[1] I have since had no reason to change this opinion so far as reduction of one's own observations are concerned. When it is a question of combining a large number of observations by different observers, as is necessary in Coast Survey work, the case is somewhat different.

[2] As judged from the divergence of the observations from a smooth curve.

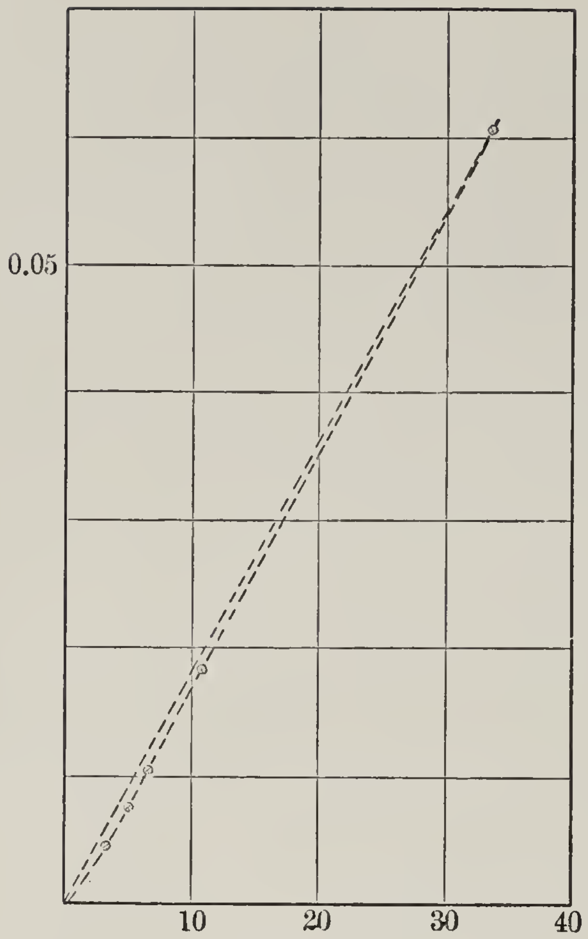

Fig. 3.

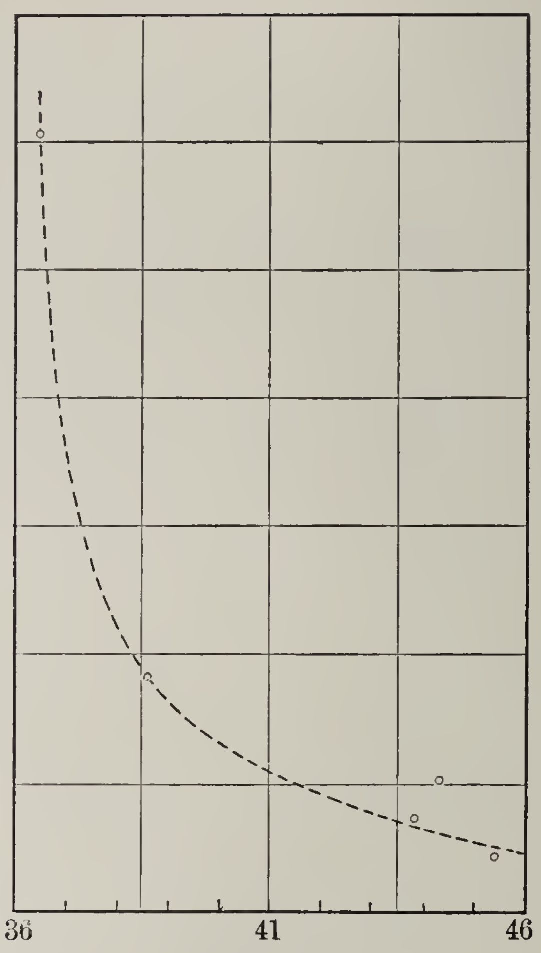

Fig. 4.

A change in specific resistance is what we might expect with decrease of thickness, because of the effect of one surface upon the other, particularly when the thickness of the film approaches the range of molecular attractions. No such limit has yet been reached here; yet it would seem that such an effect is beginning to be felt. It is possible that these results may be explained by considering that the effective thickness of the oil film is greater than the thickness of the supporting pieces on account of the cavities due to the scraping of the plates, although such an effect is directly opposed to the effect of causes (a) and (c), (see p. 80) both of which tend to *decrease* the thickness of the film. In order to determine whether this explanation is sufficient to account for the differences I have observed, I have assumed that the effective thickness of each film is $a+x$ where x is the mean correction required to the measured thickness a, on account of the cavities in the plates. To determine x we take the two extreme values of Q_{20} given in Table VI., and equate the values of σ obtained from them. This gives us

$$\begin{array}{ccc} \text{Series B.} & & \text{Series E.} \\ \dfrac{33.635}{0.0608+x} & = & \dfrac{3.18}{0.00462+x} \end{array}$$

Whence $x = 0.00125$

For the corrected thickness and specific resistance we then obtain

Series.	Corrected thickness.	Corrected sp. resist. Megohms per cu. cm.	δ
B	0.″06205	35750	0
A	0.01938	36200	450
C	0.01146	39400	3650
D	0.00875	37500	1750
E	0.00582	35750	0

From this table it will be seen that while the first and last results are reconciled by this assumption, all the others are still large, and what is more, the divergence is systematic and symmetrical. If the differences were fully accounted for by the assumed correction x to the thickness, the positive and negative differences from the mean ought to have been irregularly distributed.[1]

The most conclusive way, however, of determinating whether this explanation is the true one or not is to use two surfaces which have been ground together after scraping, so as to obtain two perfectly *smooth* as well as *true* surfaces. Unfortunately, I was not able to do this, because the plates were the property of the mechanical department. As soon as possible I intend to make a pair of plates for myself, and repeat the experiments.[2] In conclusion I wish to express my thanks to Professor Thomas for the use of his instruments and laboratory, and to Professor Robinson for the loan of the surface plates.

---

[1] A further objection to this method of explaining the observed variation in σ_0 is the large value of x necessary. According to Reynolds (see paper "On the Theory of Lubrication, and its Application to Mr. Tower's Experiments," Phil. Trans., Vol. 177, § 18, p. 184), the irregularities in a well scraped surface ought not to be greater than 0.″0001, and the maximum correction, therefore (supposing the hollows in the two plates to all come opposite each other! ! !), ought not to be more than 0.″0002 at the most; only 1/6 that which it is found necessary to assume.

[2] When I spoke of these results to Professor Michelson about a year after they had been obtained, he told me that he had found a similar result in the case of gold or silver films, (I have forgotton which), *i. e.*, that the specific resistance of the metal as determined from these thin films was very much higher than the values obtained from wires or strips. This strengthened my belief still further in the reality of the observed change. Still further evidence in this direction has been afforded by the recent results of Reinhold and Rücker on the electrical resistance of soap films. In their first work on this subject these authors found (see Phil. Trans., 1883, Section A.) that in soap films made from a solution containing 3 per cent. of KNO_3 in solution, the specific resistance "is the same whether the liquid be examined in bulk or in the form of a film 12$\mu\mu$ (0.″0000005) in thickness." But more recently (Proc. Royal Soc., Vol. 53, p. 304) they have found that when the salt is omitted from the solution, the specific resistance changes with change in thickness, although in this case it diminishes, *i. e.*, the electrical conductivity increases, as the film grows thinner.

Physical Laboratory, Ohio State University,
Columbus, December, 1888.

Notes on the Determination of the Thickness of the Film of Oil in the Bearings of Revolving Shafts.

In 1888, while working in the mechanical laboratory of the Ohio State University, the question came up as to the actual thickness of the film of oil in the bearings of a shaft rotating at different speeds.[1] At that time I was unable to find any experimental data on the subject, and as the question seemed one of considerable practical as well as theoretical interest, I spent some time in devising plans by which the problem might be experimentally attacked under actual working conditions. Of the methods which suggested themselves at the time, that which seemed the most promising and practical was to measure the electrical resistance between the box and the shaft when the latter was in motion and at rest. In the latter case the shaft is practically in metallic contact with its box, and the resistance is very low; but in the latter it is, according to Reynolds' results, always separated from it by a thin film of oil, whose thickness and therefore whose resistance will depend on the speed of the shaft and the pressure on the box. In order to determine from the measured resistance the thickness of the film it is necessary to know the specific resistance of the oil, and the temperature coefficient (to determine which the experiments described in the preceding paper were undertaken), and also the area over which the film is spread and its temperature.

It is further necessary to know whether the thickness of the film is uniform, or if not, in what way it varies.

As regards the temperature of the oil, this is easily determined by means of a thermometer placed in a deep cavity in the metal of the box or shaft as near the bearing surface as possible, the cavity being filled with water, or preferably with mercury. The mass of the metal surrounding the bearing surfaces is so great in comparison with the mass of the oil, and its conductivity is so high that the temperature recorded by the thermometer could be but very little if any lower than the temperature of the oil film itself, which is the seat of the heating.[1] The area of the film is, of course, the total area of the bearing between box and shaft. The point most difficult to determine is as to whether the film is of equal thickness or not, over the whole of the bearing surface. Two distinct causes, which may be classed respectively as irregular and regular, tend to introduce differences in thickness between different parts of the oil film. The first includes such irregularities as are due to roughness or inequalities in the surfaces of the box or shaft, or to the introduction of particles of metal by abrasion, etc. The first cause of irregularity may be avoided by choosing for the experiments a shaft and journal box which have first been carefully fitted to each other, and then allowed to run together, first in one direction then in the other, a sufficient length of time under light pressure to produce perfectly smooth and polished surfaces; the second by carefully cleaning the box and shaft before beginning the experiments, with benzine, and then during the measurements keeping the bearing well supplied with clean, fresh oil from a suitable reservoir.

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[1] See paper by Professor Osborne Reynolds, Theory of Lubrication, etc., already referred to, (foot note on p. 93).

The regular causes of variation in thickness are, first, the differences in pressure in different parts of the journal box. The effect due to differences in pressure can be very nearly eliminated for the purposes of experiment by applying a pressure to the top box which is equal to the pressure exerted on the box by the weight of the shaft and attached mechanism, or by using, as was done in this case, in the experiments which were begun, a Thurston oil testing machine,[2] in which the journal brasses are brought together by a spring which acts simultaneously and equally on both top and bottom brasses. A further advantage of using this machine is that the work lost in friction and therefore the heat developed in the box in any given length of time may be accurately measured in each experiment. The second cause of variation, *i. e.*, that due to eccentricity of the shaft in the box, is produced by the variation in the thickness of the film itself. If the bearings are perfectly fitted to each other when they are in actual contact (see Fig. 5), then when the two are separated slightly the opening between the two is greater at the bottom *a* than at the top *b*. In practice, however, the two are more likely to be concentric when slightly separated than when actually together, because that is the condition under which wear between the two takes place. Hence if, as is nearly always the case, the box is softer than the shaft and the two are allowed to run together any length of time at a given pressure equally applied to both boxes, the surfaces will at that pressure be nearly if not quite concentric, particularly if the sides of the box are slightly relieved as shown in dotted lines in Fig. 5, and the direction of motion is frequently reversed.[1]

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[1] This last quantity may also be indirectly determined from measurements of the frictional resistance, *f*, in the manner indicated by Reynolds, loc. cit.

[2] For description see Professor Thurston's "Friction and Lost Work in Machinery and Mill Work," John Wiley & Sons, New York; also Pratt & Whitney's Catalogue. 1893 ed., pp. 289–91.

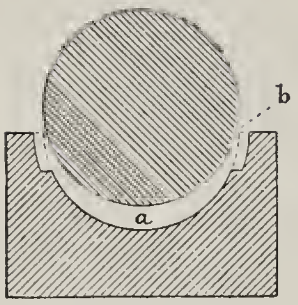

Fig. 5.

As has already been stated, these experiments were interrupted before the apparatus for the resistance measurements had been set up and put in working order. Some preliminary results, however, had been secured by placing an ordinary (comparatively insensitive) tangent galvanometer directly in series with a resistance box, and the revolving shaft, a single gravity cell being used to furnish the current.

Qualitative results were secured, showing that the resistance was measurably increased even when the pressure was very great and the speed very slow.[2] The deflection of the galvanometer decreased very rapidly (showing rapid increase of resistance) when the speed was raised or the pressure diminished. Under any one condition the deflection was as constant as could have been expected, considering the conditions of the experiment, there being at the time no provisions for a constant supply of oil or for the maintenance of constant speed.

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[1] Further consideration respecting this problem will be found in another paper by the writer "On the Theory of Lubrication and the Determination of the Thickness of the Film of Oil in Journal Bearings," which will be published in the October or November number of the Journal of the Franklin Institute.

[2] This confirms Reynolds conclusion that a film of oil is always present between the moving surfaces.

ADDENDUM.

While writing these notes another plan of measurement has occurred to me which would be useful as a check on results obtained by the electrical method, although it has not the advantage of the latter in allowing results to be obtained under the same conditions as usually obtain in machine bearings. This would be to have a long conical bearing, Fig. 6, accurately fitted to a solid box *B*, whose longitudinal position with respect to the revolving shaft *A* can be accurately located by means of a microscope *C* mounted on the box, and focussed on a fine circumferential line on the shaft. The thickness of the oil film in this case is determined by the longitudinal motion of *B* on *A*. Differing pressures between shaft and box may be produced by a spring or weighted lever acting on the end of the box at *P*.

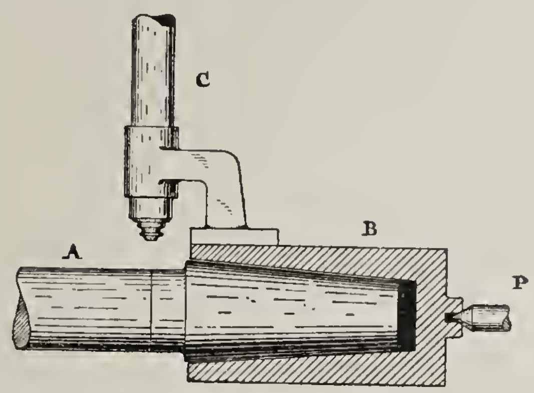

Fig. 6.

YERKES OBSERVATORY, March, 1897.

CERTAIN OPTICAL AND ELECTRO-MAGNETIC PROPERTIES OF HEAT WAVES OF GREAT WAVE-LENGTH, I.

By H. Rubens and E. F. Nichols.

IN a recent paper[1] the present writers described a method by which it is possible to obtain fairly homogeneous heat rays of great wave length without the aid of the usual spectrum apparatus. The method was based upon the principle that if the total radiation from a source emitting waves comprising a great range of different wave-lengths be reflected from a surface, the reflected rays will, in general, have a composition different from the incident rays. Waves which correspond to regions of heavy absorption in the substance of the reflecting body, and which it in consequence reflects as a metal, will have greatly the advantage over all other rays in the reflection spectrum, and in the remainder, after several successive reflections on surfaces of the same material, these rays only will be found in measurable quantity.

The matter contained in the present paper may, for convenience of treatment, be divided into four parts. The first part gives in detail the changes in the arrangement of apparatus by which a great improvement in both the sensitiveness and the accuracy of measurement was obtained. The second part contains the account of results obtained by the application of the improved method to a wider range of substances. The third part deals with the absorption reflection and refraction of long heat waves in a variety of media. The fourth part describes a number of experiments which reveal the electro-magnetic character of heat waves of great wave-length.

I.

Improvements in Apparatus.

The changes introduced in the order of apparatus described in the earlier paper were : First, the substitution of a Linnemann zirconium burner instead of the incandescent platinum foil coated with powder. By this change long waves in somewhat greater intensity were obtained, doubtless, because the zircon could be heated to a much higher temperature than the powders of such substances as fluorite for example. Second, in the interest of greater purity and homogeneity of the rays obtained, four or even five reflecting surfaces were used instead of three. Third, in place of the bolometer of the earlier study a torsion radiometer was substituted. The last two of these substitutions led to the much more complicated arrangement of apparatus shown in Fig 1.

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[1] Physical Review, IV., p. 513, 1897, and Naturw. Rundsch., 11 Jahr'g., No. 43, 1896.

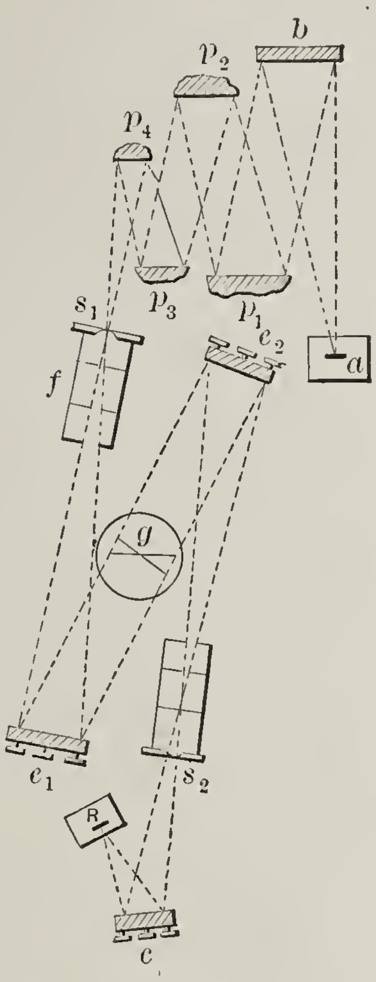

Fig. 1.

a is the zircon plate of the Linnemann burner, *b* a concave silver mirror of great focal length, $p_1\ p_2\ p_3\ p_4$ plane surfaces of the substance studied. The reflecting spectrometer $s_1\ e_1\ e_2\ s_2$, upon the table g of which a coarse wire grating could be placed for wave-length measurements, was so arranged that either a bolometer or radiometer could be used and Fig. 1 shows the arrangement of apparatus chosen for the radiometer. In this arrangement the slit s_2, in place of the eye piece or bolometer wire, remained fixed and the arm of the spectrometer carrying the mirror e_2 could be turned through any given angle and its position accurately read from the graduated spectrometer circle. An image of the slit s_2 was projected upon one wing of the radiometer at R by the aid of a concave silver mirror c of short focal length.

In this arrangement of the spectrometer the relation between the angle α, through which the mirror e_2 moves as read from the spectrometer circle, and the diffraction angle φ is not a simple one, yet a proportionality between the two for small values of α is easily proved. The course of the middle ray in the spectrometer $s_1\ e_1\ e_2\ s_2$ is shown in Fig. 2 for the zero position, and for a neighboring position of the moveable arm e_2. Let the angle between the two positions of the mirror arm be α and the corresponding defraction angle φ. Further

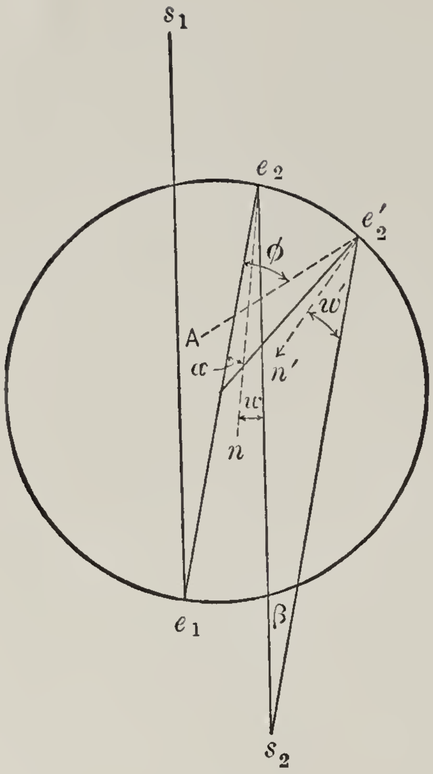

Fig. 2.

let w' represent the angle between n' the optic axis of the mirror e_2' and the ray $s_2\,e_2'$, and w represent the corresponding angle between n the optic axis of the mirror e_1 and the ray $s_2\,e_2$ then

$$w' = w + \alpha - \beta$$

where β represents the angle between $s_2\,e_2'$ and $s_2\,e_2$. The reflected ray $s_2\,e_2$ corresponds to the incident ray $e_1\,e_2$, the reflected ray e_2' s_2 corresponds to $A\,e_2'$; whence the angle φ is the difference in direction between e_1e_2 and $A\,e_2'$, and consequently

$$\varphi = \alpha + w' - w = 2\alpha - \beta$$

From this relation it appears that the assumed proportionality between φ and α holds at best only as a close approximation for small values of α. In the present case the greatest and least values of α were comprised between $+3°$ and $-3°$. The best evidence of the trustworthiness of this assumption in the present application, lies in the observation that the diffraction images occur at equal angles on either side of the central image about which they occupy symmetrical positions. The effect of a lack of proportionality between α and β would be such that for negative values of α the differential coefficient $\partial\beta/\partial\alpha$ would be greater, and for positive values less than for $\alpha = 0$. As it was impossible from the dimensions of the apparatus to establish the relation of α to β with sufficient accuracy by direct linear measurements, the spectrometer was empirically calibrated by a series of measurements in the diffraction spectra to be given later in detail.

For quite another reason the present arrangement was applicable only to small deviations. The angle of diffraction $\varphi = 2\,\alpha - \beta$ which in the present case equalled approximately 1.7α was such that for large values of α a noticeable displacement of the diffracted bundle of rays between the movable mirror e_2 and the slit s_2 occurred. As here used the maximum displacement amounted to ± 7 mm. which could not have caused any displacement of the observed energy maxima because the concave mirrors e_1 and e_2 were considerably larger than the diffraction grating so that in both extreme positions the whole diffracted bundle was caught by the mirror e_2.

This arrangement of the spectrometer is without doubt less rigorous than that proposed by Wadsworth[1] which likewise permits both slits to remain fixed, yet the simplicity of the arrangement here used is in its favor.

Apparatus.

The spectrometer has been described in an earlier paper by one of the present writers[2] and the grating was the same one used in the earlier study. Suffice it here then to say that the circle of the spectrometer could be read to seconds, and the grating constant, as optically determined and as directly measured on the dividing engine, was 0.3716+mm. The grating from its peculiar construction gave only spectra of the odd orders.

By the aid of an eye-piece mounted directly behind the fixed slit s_2, with the grating on the spectrometer table at g, the empirical calibration of the spectrometer was made by reading the settings for the images of the sodium line on both sides of the central image. The results are shown in Table I.

TABLE I.

Order of Spectrum.	Deviation observed		$\frac{r+l}{2}$ observed.	Computed.	Difference δ''
	left l	right r			
5	16′ 17″	16′ 23″	16′ 20″	16′ 20″	0
9	29 45	29 45	29 45	29 42	−3
13	43 0	42 45	42 57	42 54	−3
17	56 20	56 0	56 10	56 6	−4
21	69 25	69 10	69 17	69 18	+1
25	82 35	82 20	82 28	82 30	+2
29	95 55	95 30	95 42	95 42	0
33	109 5	108 35	108 50	108 54	+4

The numbers in the table show that beyond the image of the 13th order a systematic asymmetry appears between the position of the images on the right and the corresponding images on the left, which in the case of the image of the 33d order reaches 30″ of angle, but the mean values of the images right and left still agree perfectly with the computed values. The computed values correspond to the diffraction of a grating with a constant of 0.6139 mm. The ratio of this apparent constant to the real constant 0.3716 is 1.6522— a factor with which the observed angles of deviation α are to be multiplied to obtain the true diffraction angle φ. The magnitude of this factor depends on the position of the fixed slit s_2, it was, therefore, necessary to determine the position of this slit from time to time. The uncertainties produced by the slight changes in position thus observed affected the values obtained by quantities less than 0.2 per cent.

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[1] Wadsworth, Phil. Mag., 49, p. 337, 1894.

[2] H. Rubens, Wied. Ann. 54, p. 270, 1894.

Instead of the grating described above, a similar grating of higher dispersion, made of gold wires, was often introduced, the constant of which was 0.0662+ mm.; but the inferior quality of this grating, when compared with the former one, rendered the results obtained by its aid even less trustworthy.

The torsion radiometer resembled closely the instrument previously described by one of the present writers.[1] In the new radiometer no fluorite window was used, but instead a plate of chloride of silver 2½ mm. thick in place of the mica plate. The chloride of silver was found to transmit rays of 24 μ wave-length partially.[2] The new radiometer vanes were very much smaller than in the old suspension—measuring only 6 mm. long by 1.5 mm. wide and 5 mm. across from the outer edge of the one vane to the outer edge of the other. The vanes were coated with finely powdered black oxide of iron. The disturbance of a very troublesome tremor of the radiometer suspension encountered at first, due to a tremor of the building, was completely excluded by suspending the instrument as a whole from a wall-bracket according to the method of W. H. Julius.[3] It was further possible to so completely screen the radiometer from outside thermic disturbances that the single throws, measured on a scale 3,000 scale divisions from the instrument, were trustworthy to 0.2 scale division. In spite of the heavy absorption of the chloride of silver radiometer window for the long waves the radiometer proved to be approximately three times as sensitive as the platinum bolometer, earlier described. If the differences shown by the two instruments in freedom from extraneous disturbances be taken into account the effective sensitiveness of the radiometer was more than five times that of the bolometer. In spite, however, of this great gain in sensitiveness of the radiometer over the bolometer, the radiometer could only be used after some material for a window had been discovered by the aid of the bolometer which transmitted partially, at least, the radiations in question.

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[1] E. F. Nichols, PHYSICAL REVIEW, IV., p. 297, 1897; also Berl. Ber. 5, Nov., 1896 and Wied. Ann., 60, p. 401, 1897.

[2] The diathermancy of chloride of silver for waves of great wave-length was first discovered by C. Schulze-Sellack, Pogg. Ann., 139, p. 192, 1870. The plates used here were selected from among those made and used by Schulze-Sellack.

[3] W. H. Julius, Wied. Ann., 56, p. 151, 1895.

II.

The Application of the Method.

Preliminary studies of the substances fluorite and quartz have been communicated in an earlier paper.[1] In quartz another region of metallic reflection has been found which was overlooked in the earlier study, owing to the presence of a rocksalt lens (which completely absorbed the rays of the second maximum) used to concentrate the rays from the lamp on the reflecting surfaces. The study of the short spectrum resulting from multiple reflection on fluorite surfaces has been more fully carried out with the improved apparatus which has led to more accurate results.

Substances Studied.

The method has so far been extended to the study of eleven substances including quartz, mica, fluorite, rocksalt, sylvine, crown glass, flint-glass, sulphur, potassium alum, shellac, and both rays in Iceland spar. In the first three substances regions of metallic reflection were found and the distribution of energy in the reflection maxima determined. Rays were obtained after five reflections from rocksalt, which were all but totally absorbed by the interposition of a very thin plate of the same substance. Owing to the very low intensity of these rays we have not yet succeeded in finding the position of the energy maximum in the diffraction spectrum.

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[1] Rubens & Nichols, l. c.

Quartz.

The first region of metallic absorption in quartz is not so far removed from the energy maximum in the spectrum of the zirconium lamp as to make measurements of the distribution of energy in the first spectrum of rays after four-fold reflection on quartz surfaces, difficult. With the radiometer and slit widths varying from 1 to 1.5 mm. it was even possible to trace the energy curve of these rays in the higher dispension of the spectrum of the third order.

The results of such a series of observations are shown graphically in Fig. 3, in which abscissas represent angle readings of the movable mirror arm and ordinates the corresponding throws of the radiometer. In this series the slits s_1 and s_2 were 1.5 mm. wide. The greatest throw obtained in the energy maximum of the central image was 325 mm.

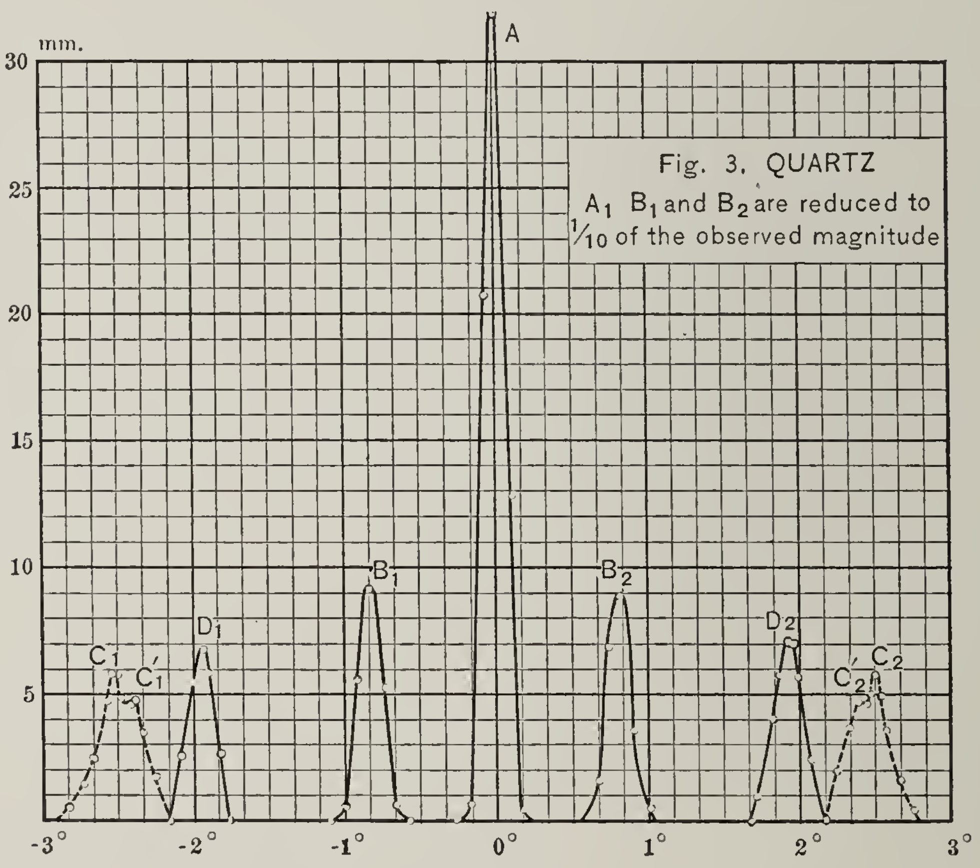

Fig. 3.

Fig. 3 shows, in all, nine maxima. Of these *A* is the central image, B_1, B_2 the images of the first order of the absorption band previously observed; the corresponding images in the spectrum of the third order are seen in C_1 C_1' and C_2 C_2' as dotted curves on the extreme ends. Of these the outermost C_1 and C_2 are the more intense.

In addition, the figure shows two sharply defined maxima D_1 and D_2. When a plate of rocksalt 2.6 cms. thick was interposed, the energy corresponding to these bands vanished completely while the ordinates of B_1 and B_2, C_1 C_1' and C_2 C_2' were reduced by about 10 per cent. This explains the reason why D_1 and D_2 did not appear in the results of the earlier study where, as already mentioned, a rocksalt lens was used for concentration. The distances of the maxima on either side of the central image and the wave-lengths corresponding to the settings of the spectrometer are given for two complete series of measurements in Table II. The second series does not elsewhere appear in detail.

TABLE II.

Maximum.	Angle of deviation α.		α Mean.	Wave-length λ
	Series I.	Series II.		
B_1	49′	50′	49′ 30″	8.85 μ
B_2	50	49		
C_1	2° 31½′	2° 32′	2° 31 38	9.02
C_2	2 31	2 32		
C_1'	2 24½	2 23	2 23	8.50
C_2'	2 22½	2 22		
D_1	1 56½	1 57	1 56 30	20.75
D_2	1 56½	1 56		

In accordance with these results quartz is seen to possess narrow regions of metallic reflection in the infra-red spectrum the maxima of which correspond to $\lambda = 8.50\mu$, 9.02μ and 20.75μ respectively.

The first two bands which lie close together were earlier discovered by one of the present writers.[1] The values of λ obtained at that time were 8.42μ and 8.80μ respectively of which the one is less by 1 per cent., and the other by 2.7 per cent. than the corresponding values just given. This difference is doubtless due to the increasing impurity with increasing wave-length in this region of the spectrum of the fluorite prisms in which the earlier study was made. The reason for this exists in the heavy and increasing absorption of fluorite which begins in the region under discussion. The effect of this sort of impurity would be to displace both maxima toward shorter wave-lengths, and for the same reason the second would suffer greater displacement than the first. This view is further confirmed by the fact that the second maximum C_1 C_2 in the present study is seen to be higher than the first, while the reverse was found in the earlier measurements. The possibility of such an error was foreseen, and attention called to it in the earlier paper.[1]

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[1] E. F. Nichols, l. c.

The Ketteler-Helmholtz dispersion formula, when applied to early measurements of dispersion in quartz, placed the middle of the infra-red absorption band, assumed single, at $\lambda_2 = 10\mu$.[2] In this computation the form

$$n^2 = b^2 + \frac{M_1}{\lambda^2 - \lambda_1{}^2} - \frac{M_2}{\lambda_2{}^2 - \lambda^2} \qquad (1)$$

was used.[3] It is quite possible that the new absorption band at $\lambda = 20.75\mu$ is not wholly without influence upon the dispersion in the early infra-red, or even in the visible spectrum. At any rate it is impossible to make equation (1) fit the early observations even approximately by substituting any value for λ_2 between 8.5 and 9μ where observation has placed it. On the other hand an attempt to introduce a third term of the same form as the previous two on the right side of equation (1) was more successful. With this addition the formula stands

$$n^2 = b^2 + \frac{M_1}{\lambda^2 - \lambda_1{}^2} - \frac{M_2}{\lambda_2{}^2 - \lambda^2} - \frac{M_3}{\lambda_3{}^2 - \lambda^2} \qquad (2)$$

In which $\lambda_2 = 8.85\mu$— the wave-length of the approximate center of absorption as shown by the mean value of B_1 and B_2 in Table II. λ_3 was put equal to 20.75μ— the wave-length of the middle of the absorption band D_1. Equation (2), while it contains the same number of arbitrary constants earlier used,[4] is in harmony with the new measurements as well as with the old.

Table III contains the observed values of the refractive index for a number of wave-lengths in the ultra-violet, visible, and infra-red spectrum which are compared with corresponding values computed by equation (2); in which the constants M_1 and λ_1 are those computed by Ketteler,[1] and b^2 was obtained by the aid of the equation,

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[1] E. F. Nichols, l. c. Phys. Rev., p. 308.

[2] H. Rubens, Wied. Ann. 54, p. 476, 1895.

[3] Wherein n is the index of refraction, λ the wave-length and b^2, M_1, M_2, $\lambda_1{}^2$, and $\lambda_2{}^2$ are arbitrary constants.

[4] In this form the arbitrary constants are M_1, M_2, M_3, b^2 and λ_1.

$$b^2=a^2+\frac{M_2}{\lambda_2^2}+\frac{M_3}{\lambda_3^2}$$

and the value a^2 given by Ketteler.[2] The two remaining constants M_2 and M_3 were computed from the observed indices for wave-lengths 2.84μ and 4.20μ.

Table III.[3]

Wave-length λ	n obs.	n comp.	Diff. $\delta . 10^4$	Wave-length λ	n obs.	n comp.	Diff. $\delta . 10^4$
0.19881	1.65070	1.65077	+0.7	1.969	1.5216	1.5216	0
0.27467	1.58750	1.58757	+0.7	2.84	1.5039	1.5039	0
0.35818	1.56400	1.56395	−0.5	3.63	1.4799	1.4801	+2
0.4341	1.55387	1.5539	0	4.20	1.4569	1.4569	0
0.5350	1.54663	1.5466	0	5.0	1.417	1.412	− 50
0.6563	1.54181	1.5419	+1	5.8	1.368	1.339	−290
0.7682	1.53893	1.5390	+1	6.45	1.274	1.241	−330
1.160	1.5329	1.5329	0	7.0	1.167	1.096	−710

$b^2 = 4.57877$

$M_1 = 0.010654$ $M_2 = 44.224$ $M_3 = 713.55$

$\lambda_1^2 = 0.010627$ $\lambda_2^2 = 78.22$ $\lambda_3^2 = 430.56$

The agreement between values of n computed from (2) and those obtained indirectly—from observation of reflection—is not close, yet the divergence would be fully explained by the assumption of the presence of from 1 to 2 per cent. of the energy belonging to $\lambda = 1.4\mu$ in this part of the spectrum, or of 0.3 per cent. of stray rays from $\lambda = 8.5\mu$. The former wave-length corresponds approximately to the energy maximum in the spectrum of the zirconium burner, the second to the middle of the neighboring absorption band in quartz.

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[1] E. Ketteler, Wied. Ann. 30, p. 312, 1887.

[2] Compare H. Rubens, Wied. Ann. 53, p. 268, 1894.

[3] The dispersion measurements between wave-lengths $\lambda = 0.19881$ and 0.35818μ are taken from Sarasin, between $\lambda = 0.4341$ and 4.20μ from Rubens and from $\lambda = 5.\mu$ to $7.\mu$ from Nichols.

A second and more probable reason is doubtless to be found in the particular form of equation (2) which makes it inapplicable in the immediate neighborhood of a band of metallic absorption. As early as $\lambda = 8\,\mu$ the formula gives negative values for n^2 so that the limitations of the formula would appear to be the more reasonable explanation of the disagreement.

Mica.

Doubtless on account of the large percentage of silica[1] which enters into the composition of mica, rays after four reflections or surfaces of this substance yield a spectrum which resembles strikingly that of quartz just described. Fig. 4 contains the results of a series of observations of rays sent out from the zirconium lamp which, after four reflections on plane surfaces of mica, were analysed by the grating and radiometer. In this series the slits s_1 s_2 were opened 2½ mm. wide.

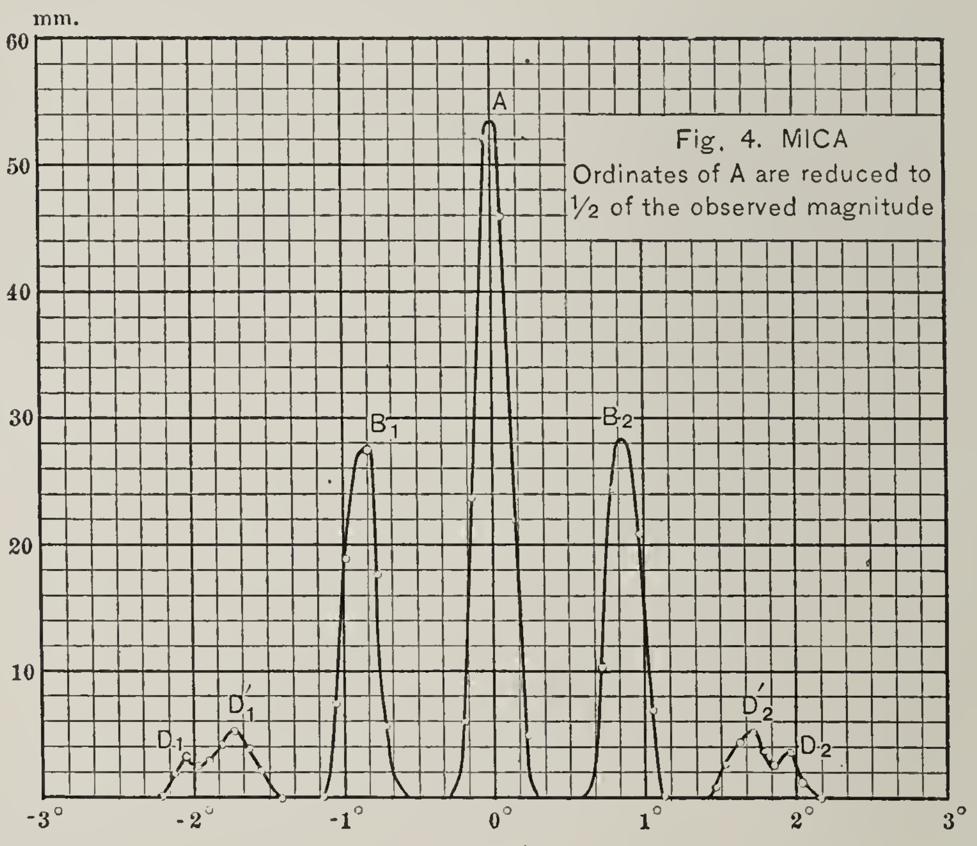

Fig. 5.

In addition to the central image A the curve shows three maxima on either side, where regions of metallic reflection exist. That the images all belong to the spectrum of the first order, and that D_1 D_1' and D_2 D_2' are not repetitions of B_1 and B_2 in the spectrum of the third order, is that the former vanished when a rocksalt plate 2.6 cm. thick was introduced into the path of the rays. Table IV gives the exact positions and the corresponding wave-lengths for the several maxima.

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[1] According to the observations of Rammelsberg mica contains from 45 to 47 per cent. of silica (Neumann, Mineralogie).

TABLE IV.

Maximum.	Angle of deviation α	α Mean.	Wave-length. λ
B_1	51′	51½′	9.20 μ
B_2	52′		
D_1	2° 0′	1°59′	21.25
D_2	1 58		
D_1'	1 43	1 43′	18.40
D_2'	1 43		

With the exception of the maximum at $\lambda = 18.40\mu$, the absorption bands correspond very closely to those observed in quartz. Because of the wide slits, and the low dispersion of the grating it was impossible to draw any conclusion concerning the distribution of energy in the maxima B_1 and B_2.[1] In consequence, these two bands were compared with the corresponding quartz bands by the aid of the grating of gold wires already mentioned which gave a higher dispersion. The results thus obtained are shown in Fig. 5.

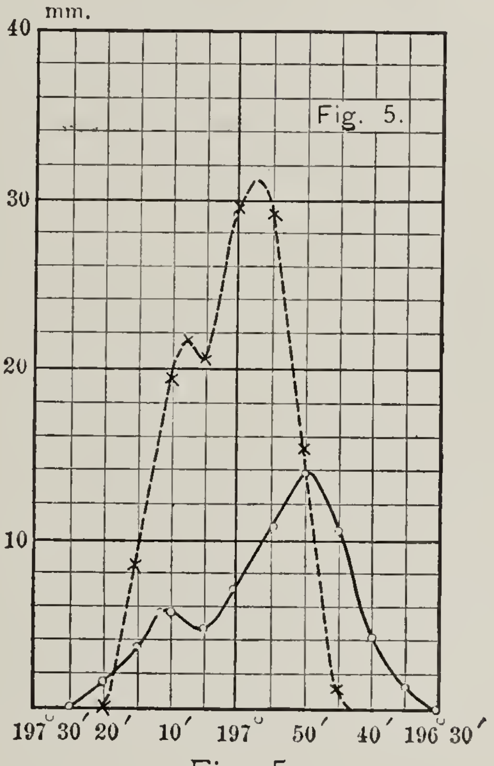

Fig. 5.

The lower curve gives the distribution of energy in the mica band, and the dotted line the same for quartz under the same circumstances. The setting for the central image in both cases was 199° 55′ 0″. From Fig. 5 it appears that the reflective power of quartz in this region is much greater than that of mica. It is further seen that the elevations B_1 and B_2 consist of two separate maxima, the corresponding wave-lengths of which do not agree with the similar maxima in quartz. The wave-lengths of the maxima in mica are 8.32 μ and 9.38 μ, and those for quartz 8.52 μ and 9.03 μ respectively. The last two values are in close agreement with the results of the quartz series given in Table II.

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[1] The images in the spectrum of the third order could not be studied because too weak in energy.

The natural conclusion from these results is, that the metallic reflection of mica, with perhaps the exception of the maximum at 18.40 μ, is due to the silica which it contains. The small differences in the position of the reflection maxima are doubtless wholly due to an effect, already well known through optical studies—the influence of the solvent upon the absorption spectrum of the substance dissolved.

Fluorite.

The apparatus and method used in the earlier determination of the metallic reflection of fluorite have been fully described elsewhere.[1] In the present repetition of that study the following essential changes have been introduced, which require attention: First, the source of energy has been changed from an incandescent sheet of platinum coated with powdered fluorite to a zirconium lamp. Second, the number of reflecting surfaces was increased from three to four. Third, the arrangement of the spectrometer was changed. Fourth, the radiometer with vanes coated with black oxide of iron was substituted for the bolometer coated with platinum black. Fifth, a plate of vitreous chloride of silver, in the form of a radiometer window, was introduced into the path of the rays.

The results obtained with the present arrangement are shown in Table VI, while those obtained earlier appear in Table V.

TABLE V.

Series.	Deviation from central image.		Wave-length λ
	Right.	Left.	
I	3° 45′	3° 41′	24.2 μ
II	3 48	3 47	24.7
III	3 44	3 46	24.4
		Mean	24.4 μ

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[1] Rubens and Nichols, l. c.

TABLE VI.

Series.	Deviation from central image.		Wave-length λ
	Right.	Left.	
I	2° 13′	2° 13′	23.8
II	2 12	2 13	23.7
III	2 12	2 13	23.7
			Mean 23.73

A difference appears between the earlier values and the later ones which lies altogether beyond the range of error in either case. The ground of this discrepancy was found to be a strongly selective absorption of chloride of silver. This absorption changes markedly even within the narrow limits of the short spectrum lying between 22 and 28μ. At three different points within this range, the transmission of a second silver chloride plate was studied and found to be

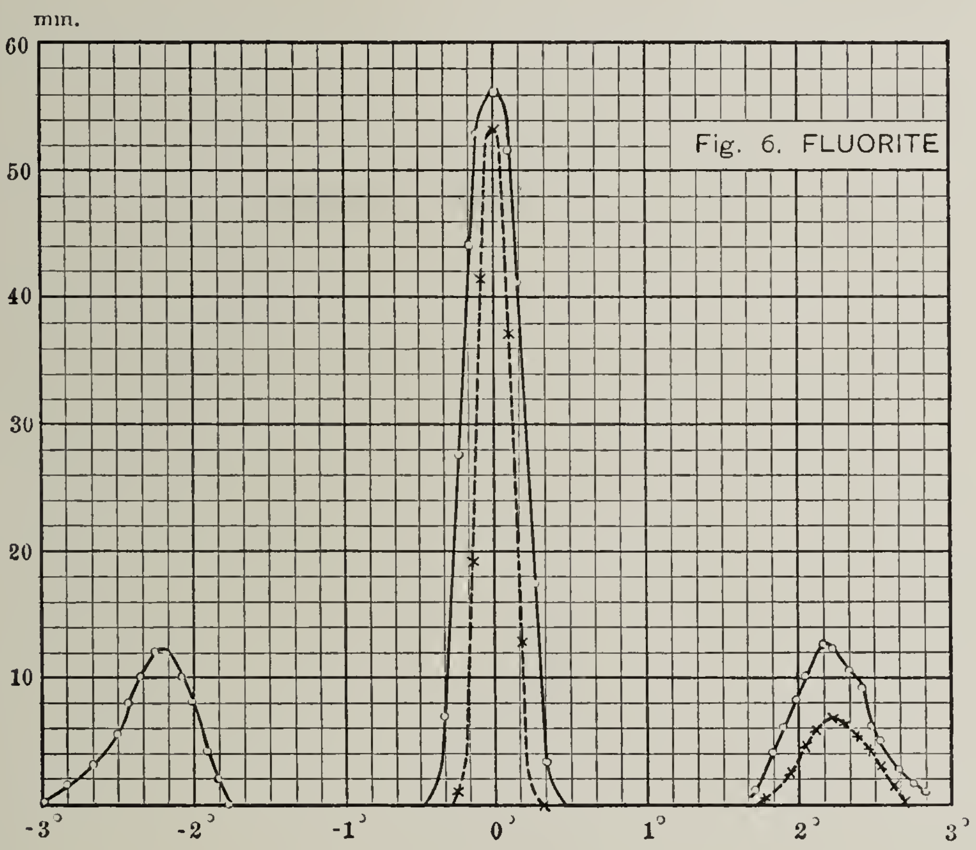

Fig. 6.

40 per cent. at $\alpha=2^\circ\ 10'$, 37 per cent. at $\alpha=2^\circ\ 15'$, and 33 per cent. at $\alpha=2^\circ\ 20'$. The combination of this transmission curve with the curve representing the true distribution of energy in the reflection maximum would be such as to displace the latter toward shorter

wave-lengths by approximately 0.°3′30″ of angle which corresponds to a wave-length difference of 0.62μ. This, when added to the observed maximum 23.73μ, gives 24.35μ as the true position of the maximum which agrees very well with 24.4μ, the value reached in the earlier study.

The first series of observations in Table VI. is shown graphically in Fig. 6. The slit widths were 3.5 mm. The radiometer throw for the middle of the central image was 56 mm., and in the maximum of the image of the first order 12 mm. was obtained. The distribution of energy is better shown by another series of observations made with slits 2 mm. wide, the results of which appear in the dotted curve in Fig. 6, which, from its form, suggests the possibility that the observed maximum consists, in reality, of two maxima belonging to a double band as in the case of quartz and mica. An attempt to effect the further separation of the two maxima was unsuccessful on account of the small quantity of energy present in these rays.

(*To be concluded.*)

MINOR CONTRIBUTION.

On the Relation Between the Osmotic Pressure and the Vapor Pressure of Solutions.

By A. A. Noyes and C. G. Abbot.

1. Introduction.

IT is well known that the direct measurement of osmotic pressure is attended with great difficulty and consequently the few experiments[1] which have been carried out for this purpose have led to hardly any accurate and reliable results. Recourse has therefore been had to thermodynamic relations which make possible the calculation of this highly important quantity from more accessible data. In this way the diminution of vapor pressure, the lowering of freezing point, and other properties have been made use of. But as Nernst[2] has recently pointed out, all these thermodynamic relations give directly the osmotic work, and not the osmotic pressure itself. Since the so calculated osmotic work is the product of the osmotic pressure by a volume change which can not be experimentally measured, namely, the volume change which occurs when a definite amount of solvent is added to an infinite amount of the solution, the determinations heretofore made of osmotic work have not yielded reliable values for the osmotic pressure. The osmotic pressure has, to be sure, been calculated from the osmotic work by various investigators, but invariably with the help of certain assumptions. Thus, for example, Arrhenius[3] and Dieterici[4] have sought to determine the volume factor by investigating the volume change that occurs when to a *finite* amount of solution a certain definite amount of solvent is added. The fact that the contraction is dependent in a high degree on the quantity of added solvent should have furnished a warning against this mistake. By other authors[5] it has been assumed quite as arbitrarily that the volume change of the solution is approximately identical with the volume of the added solvent. Nernst[6] and Abegg[7] appear, on the other hand, to be aware of the arbitrary nature of these assumptions, and the latter entirely refrains completely from calculating the pressure from the work.

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[1] *Pfeffer.* Osmotische Untersuchungen. *Adie,* J. Chem. Soc., *1891,* 344.

[2] Theoretische Chemie, 309.

[3] Zeitschr. für Phys. Chem., *10,* 92.

[4] Wied. Ann.

[5] Schilling, Program der K. K. Staatsoberrealschule in Olmutz für 1893–4.

[6] Theoretische Chemie, 209.

[7] Zeitschrift für Phys. Chem., *15,* 250.

It is the purpose of this article to show from the principles of energy how osmotic pressure itself can be calculated from the diminution of vapor pressure, and to deduce the relation between it and osmotic work.

2. *Derivation of the Relation Between Osmotic Pressure and Vapor Pressure.*

Previous deductions of the relation between osmotic work and vapor pressure have all been based on the consideration of the energy changes involved in an isothermal reversible cyclical process or a virtual change of the equilibrium. For the purpose of deriving a relation involving the osmotic pressure, we will make use of the second law of thermodynamics in another form, namely, that which asserts that in a condition of equilibrium there can be no uncompensated intensity differences.[1]

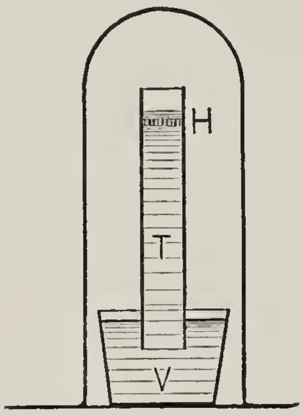

Fig. 1.

Consider the system sketched in the accompanying figure and the case of equilibrium there represented. The vessel V contains a pure solvent which also fills the tube T up to the point H. At this point is placed a semipermeable wall, upon which rests a very thin layer of a solution in the same solvent. When equilibrium is reached, which comes about by a change in the concentration of the solution caused by the entrance or expulsion of solvent through the semipermeable wall, the osmotic pressure P of the solution (which in the mathematical discussions is commonly defined as the total difference in pressure between the solvent and solution[2] must evidently be equal and opposite to the hydrostatic pressure. The latter is equal to the weight of the column of solvent in the tube T diminished by the vapor pressure p_0 of the solvent and increased by that of the solution p. That is to say, if s be the specific gravity of the solvent and h_1 the height of the osmotic column above the surface of the liquid in the vessel V:

$$P = -(p_0 - p) + \int_0^{h_1} s \, dh.$$

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[1] See Ostwald, Zeitschr. für Phys. Chem., 10, pp. 366, 379.

[2] This customary definition appears to us to be an unfortunate one, for, according to it, the osmotic pressure is not a single physical quantity but the algebraic sum of three independent quantics, namely, the true osmotic pressure which is exercised on the semipermeable wall by the dissolved molecules, the vapor pressure of the solvent and that of the solution which three things, for example, follow quite different temperature laws. A second disadvantage of the definition consists in the fact that the energetic relations with other quantities cannot be so simply expressed as if the rational definition of osmotic pressure was adopted. Since, however, the two quantities are different only by an amount negligible for practical purposes, we retain the customary definition to avoid confusion.

The specific gravity may be expressed with almost absolute accuracy, as follows:

$$s = s_0 - \frac{ds}{dh}(h_1 - h)$$

where $\frac{ds}{dh}$ is considered constant and s_0 is the specific gravity of the solvent at the top of the column, or practically its value under the usual pressure.

By integration, introduction of the compression coefficient $k = \frac{ds:s}{dP}$ and consideration of the fact that in the term of the second order expressing the effect of compression, it can be assumed that $s = s_0$ and $P = h_1\, s$ we obtain:

$$P = h_1\, s_0\left(1 + \frac{P}{2}k\right) - (p_0 - p) \tag{1}$$

Moreover, since the system is in equilibrium the vapors from the solvent and the solution must everywhere be under equal pressure; for otherwise a distillation would begin by which the osmotic equilibrium would be destroyed and a perpetual motion would result. Now the pressure of the vapor above the solvent falls off steadily with increasing height by an amount equal to the weight of the intervening column of vapor. That is, if δ be the specific gravity of the vapor

$$dp = -\delta dh$$

or

$$h_1 = \int_p^{p_0} \frac{dp}{\delta} = \frac{1}{M}\int_p^{p_0} v dp \tag{2}$$

where M denotes the molecular weight and v the molecular volume of the vapor of the solvent. If, therefore, h_1 denote as before the height of the osmotic column, and p the vapor pressure of the solution, substituting for h_1 in equation (1) we obtain:

$$P = \left(s_0 + \frac{P}{2}k\right)\frac{1}{M}\int_p^{p_0} v dp - (p_0 - p) \tag{3}$$

In this expression all the quantities of the second order are retained. In almost all practical applications, however, these are negligible, on account of their small magnitude, in comparison with the experimental error. For example in a normal solution in ether, whose compression coefficient 0.00015 is uncommonly large, the term $\frac{P}{2}k$ is only about 0.17 per cent.

An osmotic system somewhat similar to the one here described has been already employed by Duhem,[1] Gorey and Chaperon.[2] The important difference between the two arrangements consists in that in the one case the semipermeable wall is at the top of the column and in the other it is at the bottom. The latter arrangement is unsuitable for determining the relation between osmotic and vapor pressure because the concentration of the solution varies continuously from the bottom to the top of the column as shown by Lorenz[1] and more plainly by Guglielmo,[2] thus making the osmotic and vapor pressures under consideration refer to solutions of different concentration. To neglect this consideration leads to curious conclusions, for example, that the osmotic pressure of a solution is a function of its specific gravity and that it depends on the depth of the semipermeable wall below the surface of the solvent.[3]

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[1] Journal de physique (2), 6, 136.

[2] Ann. de chim. et phys. (6), 13, 124.

3. Relation Between Osmotic Pressure and Osmotic Work.

From equation (3) determining the osmotic pressure and expressions already obtained by different authors for the osmotic work done in pressing out a molecular weight of the solvent, the relation between these quantities follows directly. The exact expression for the osmotic work Π_m is as follows:[4]

$$\Pi_m = \int_p^{p_0} v\,dp - (p_0 V_0 - pV), \tag{4}$$

in which p, p_0 and v have their previous significance V_0 is the volume of one molecular weight of the liquid solvent, and V the volume change of the solution when this amount of solvent is added to it. By uniting this equation with equation (3) remembering that $\frac{M}{s_0} = V_0$ and that in the small correction term $(p_0 V_0 - pV)$, V_0 may be assumed equal to V, the following almost exact relation is obtained:

$$\Pi_m = P \frac{V_0}{1 + \frac{P}{2} K} \tag{5}$$

Since now in this equation the member $\frac{P}{2}K$ even with quite concentrated solutions can hardly amount to one per cent, it may be said that within the limit of error of previous experiments *osmotic pressure and work are directly proportional to each other.*

A correction on account of the contraction of the solution on mixing with the solvent such as has been applied by some authors is therefore unnecessary and incorrect. Furthermore, the conclusion of Nernst,[1] based on kinetic considerations that in the calculations of osmotic pressure from osmotic work the large volume occupied by the molecules of solvent must be taken into account, is shown by this law to be incorrect.

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[1] Zeitschrift für Phys. Chem., 7, 37.

[2] Ibid, 11, 282.

[3] See Duhem (2), 6, 140.

[4] See Duhem. Jour. de Phys. (2), 6, 405; also Gorey and Chaperon, Ann. de Chim. et de Phys. (6), 13, 121.

Finally, it may be remarked that with the establishment of this law the objection raised by Abegg[2] against the contents of an earlier article by Noyes[3] falls to the ground. The objection was based on the fact that in that article, which treated of the deviations from the gas laws in solution, the freezing point determinations were regarded as a measure of osmotic pressure instead of osmotic work. This assumption is, however, warranted so long as proportionality exists between freezing point lowering and osmotic work, and that this is the case for moderately concentrated solutions, at least within the limits of experimental error, is shown by Abegg's own calculations. The law formulated in the article of Noyes that the change of the osmotic pressure with the concentration is expressed within the limits investigated by the linear equation: $P(v-d) = K$, and that the constant d depends upon the relative size of the molecules of the solvent and of the dissolved substance retains, therefore, its validity.

Boston, Massachusetts Institute of Technology,
March, 1897.

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[1] Theoretische Chemie, 209.

[2] Zeitschr. für Phys. Chem., 15, 254.

[3] Ibid., 5, 53.

NOTE.

Alfred Marshall Mayer.—Among American physicists devoted to pure science few have become so distinguished as the one whose death we are called upon now to record. After a period of intermittent ill health which lasted several years, but which did not cause a final interruption of duty until a few months ago, Professor Mayer died at his country home in Maplewood, New Jersey, on the 13th of July, 1897.

The family was of German descent, one of its members, Christian Mayer, emigrating to America in 1784, and becoming the head of a large and successful business house in Baltimore. Charles F. Mayer, the father of the physicist, was a gentleman of ample means and fine scholarship. Alfred Marshall Mayer was born November 13, 1836, and after receiving the usual school advantages afforded in every city, entered St. Mary's College in Baltimore. This he left at the age of sixteen years because he preferred scientific to classical training, and two years were then devoted to the study of mechanical engineering in the workshop and draughting room. Two more years were given to the laboratory study of chemistry, general and analytical, and at the age of twenty years the student was invited into the professor's chair to teach in the University of Maryland the subjects on which he had been specializing. During these years of special study in youth young Mayer formed the acquaintance of Joseph Henry, and a friendship was formed that continued until Henry's death twenty-five years afterward. It was Henry's influence and example which gave to the developing physicist a strong bias in favor of pure science, this term being used in contrast with industrial or commercial science, which is so much more popular everywhere, but particularly in America.

Professor Mayer accepted a chair in Westminster College, Missouri, in 1858, and remained there two years. Returning to Baltimore on the breaking out of the civil war, he remained for a time and then went to Paris, where he studied physics, mathematics and physiology. Most of his work was under the direction of Regnault. Returning to America in 1865 he accepted a call to the chair of physics and chemistry at Pennsylvania College, Gettysburg, and resigned in 1867 to take that of physics and astronomy at Lehigh University. In 1871 he went to Hoboken where the Stevens Institute of Technology was just organizing; and as professor of physics in this school of mechanical engineering the last twenty-six years of his life were spent.

During his student days in Baltimore Professor Mayer's first paper was published. It was the description of a "new apparatus for the determination of carbonic acid." This was followed in 1857 by one on the estimation of minute weights by the deflection of fine glass fibres. During the next ten years he published little, but gave his time partly to routine work and partly to laying deep the foundations for the future. His fruitful scientific activity may be said to have begun with his entrance upon work at Lehigh University in 1867, and during the next ten years this activity was at its maximum. It was at first chiefly in the channels of astrophysics, and determined by the erection of an observatory which he planned and equipped. After publishing a series of observations on Jupiter he was deputed by the national government to take charge of a party of astronomers who were stationed at Burlington, Iowa, in the path of totality of the solar eclipse of August 7, 1869. A considerable number of photographs of the eclipse were secured by him, and the discussion of results was published. About the same time, however, purely physical questions were additionally engaging his attention. The first of these was on the "Thermodynamics of Waterfalls," and was occupied with the measurement of the heat generated by such cataracts as Trenton Falls and Niagara. In 1872 he devised a method of tracing the progress of a surface of conducted heat by means of the change of color with temperature observed with certain chemical substances. He applied the same method afterward to thermographs of the solar disk. In 1890 he investigated the coefficients of cubic expansion of solids determining them by observation of the temperature at which water contained in a vessel made of the solid under examination attains the same apparent volume as it has at 0°C. Among the substances thus examined was vulcanite; and a general examination of the properties of this solid followed.

At Lehigh University Professor Mayer began also some investigations on magnetism, making many observations on magnetic declination, improving on previously known methods of fixing and photographing magnetic spectra, and devising a zero-method of comparing the strengths of electromagnets. He began afterward an elaborate research on the effect of magnetization in changing the dimensions of iron and steel, and continued this for some time. He was called upon to give frequent public lectures. One of these, entitled "The Earth a Great Magnet," was delivered before the Yale Scientific Club and subsequently published. In connection with it he devised a new and very satisfactory form of galvanometer for vertical projection which attracted much notice. Another interesting demonstration which a few years afterward attracted attention was that of the configurations assumed by floating magnets when subjected to the action of a superposed magnet.

In 1874 Professor Mayer devised a new method for the study of the oscillatory nature of the spark discharge from a Leyden jar or between the electrodes of an induction coil. Securing thus the conditions for the most nearly instantaneous discharge, he applied the induction coil with great success in the electrographic method of rating tuning forks, and in the employment of the tuning fork as a chronoscope for measuring the velocity of projectiles.

The subject of investigation in which Professor Mayer's results were most conspicuous was acoustics. His study of the tuning fork had preceded that of the spark discharge, and soon after beginning work at Hoboken he began a series of researches in acoustics which made him easily the leading authority on this subject in America. Beginning with the study of resonators, he showed how the phases of vibration in the air around a sounding body could be measured, developed a precise method of determining wave-lengths and velocities of sound in gases at various temperatures, and devised an acoustic pyrometer by which furnace temperatures were measurable with a degree of precision closely approximating that of the air thermometer. He then originated a method of determining the relative intensities of sounds under special conditions, and the absorbing power of different media for sound. The great absorptive power of India rubber led to its employment as a damper for tuning forks, and he was thus enabled to secure an approximate determination of the mechanical equivalent of an aerial sonorous vibration. He was then drawn into a study of the physiology of acoustics. He devised an ingenious method for the objective analysis of musical quality, and made some wholly new discoveries in relation to the auditory organs of the mosquito. This led to the suggestion of a modification of Helmholtz's view regarding the function of the nerve fibrils in the human ear.

Professor Mayer's most important research in acoustics was his determination of the law connecting the pitch of a sound with the duration of the residual sensation in the ear. He gave a quantitative character to what Helmholtz had reached only qualitatively.. He was thus enabled to show how musical combinations that are smooth in the upper regions of the scale become rough and dissonant in the lower parts. The work was begun in 1874, repeated in 1875, and again revised with improved apparatus and the utmost circumspection in 1893. The law is expressed by an equation in which the duration of the residual sensation is given as a function of vibration frequency, and this equation is satisfied with the highest degree of precision that could be expected in any attempt to give mathematical expression to the phenomena of sensation. The importance of this law has been recognized in the leading recent books on acoustics.

Professor Mayer successfully applied acoustic methods to investigations primarily undertaken with other ends in view. During the last few years of his life he conducted an elaborate research on the variation in the modulus of elasticity of various materials with change of temperature. From the observed pitch of a bar vibrating transversely the velocity of propagation of sound in it was found and from this the modulus of elasticity. In such a research no ordinary determination of pitch could be relied upon. He went, therefore, to Paris and secured the coöperation of Dr. Koenig, whose great tonometer is the only instrument in the world that is capable of determining pitch with severe accuracy through a sufficiently wide range. The results of the investigation showed remarkable agreement and are of great scientific value.

Other contributions to acoustics that deserve mention are Professor Mayer's discovery of what has been called acoustic repulsion and its application to the "sound mill;" an effect independently discovered afterward by Dvorâk, in Austria. He applied resonators successfully also to the location of a distant source of sound at sea, such as a fog-horn. His last acoustic paper was in 1894, on the production of audible beat tones from two vibrating bodies whose frequencies are so high that the separate tones are inaudible.

In 1893 Professor Mayer became much interested in the phenomena of simultaneous color contrast, and published a research on this subject. He contributed to the general investigation of Roentgen rays in 1896. His last work, undertaken during the intervals of the disease which terminated his life, was a study of the forces acting in the flotation of discs and rings of metal and a determination, in this way, of the capillary constant of the liquid employed. The difficulties involved were great, but the results were among the best ever attained on this subject. The research was published but a few days before a stroke of paralysis put an end to all further activity.

Of Professor Mayer's charming personal qualities little can be said in this brief notice of his scientific work. Hs was a man of great native refinement and social polish. His geniality and warmth of heart made deep impressions on all who came into contact with him. The intellectual quality most conspicuous was his extraordinary ingenuity and fertility in resources. His perception was quick, and his patience in unraveling difficulties was great. He leaves a record of originality which will last, and a large circle of friends to mourn his loss.

S.

NEW BOOKS.

Elements of Mechanics, including Kinematics, Kinetics, and Statics, with Applications. By THOMAS WALLACE WRIGHT, M.A., Ph.D., New York, Van Nostrand, 1896. 8vo. 8+372 pp.

This volume is a new and considerably enlarged edition of the author's "Text-book of Mechanics," published in 1890. The directness and simplicity with which the subject is treated throughout, the restriction of the subject-matter to what is absolutely necessary for the applications to engineering, the abundant supply of well selected exercises, questions, and "applied" problems, and a certain freshness and unconventionality of style will commend it favorably to many students and teachers. While the book has grown from 260 to 372 pages, the range and scope of the work have not been essentially widened; nor has the general method of treatment been changed very much, except in so far as the author seems to have been influenced by Professor A. G. Greenhill's unpublished *Notes on Dynamics* which we are told in the preface were placed at his disposal for the preparation of this new edition. The last mentioned fact adds a special interest to Professor Wright's volume.

The book is divided into nine chapters as follows: I. Kinematics, motion (pp. 6–43); II. Matter in motion, Newton's laws of motion (pp. 44–71); III. Dynamics of a particle (pp. 72–140); IV. Statics of a body (pp. 141–205); V. Friction (pp. 206–223); VI. Work and energy (pp. 224–270); VII. Dynamics of rotation (pp. 271–319); VIII. Elastic solids (pp. 320–337); IX. Metric units (pp. 338–367).

All questions requiring the use of three dimensions of space are rigorously excluded. Thus, even in the "Statics of a body" the six general conditions of equilibrium are not given, but only the conditions for coplanar forces; the first chapter does not extend beyond the kinematics of a point; and the seventh treats only of rotation about a *fixed* axis. For an elementary treatment this restriction to one and two dimensions is perfectly proper and very convenient as it simplifies matters enormously. The question may, however, be asked whether such a narrow treatment of mechanics is sufficient for the student of engineering for whom this work appears to be mainly designed. Even if it were admitted that the engineer never meets in actual practice with problems that cannot be reduced to the two-dimensional treatment, it would seem as if the students in our best engineering colleges ought to be given a broader view of the science of mechanics. The restriction of the theoretical part of mechanics to its narrowest limits, as we find it in such books as Professor Wright's, is due partly to the desire to avoid the use of higher mathematics, partly to the tendency to introduce numerous applications so as to make the course in mechanics really a course in *applied* mechanics. As regards the first point it may be said that the amount of pure mathematics taught in the larger engineering schools is quite sufficient for basing on it a thorough course in mechanics; indeed, it is through the course in mechanics that this mathematical knowledge is best assimilated and made so familiar as to become really useful. With respect to the second point it should be remembered that it is more important to make the student understand and appreciate the broad fundamental principles of the science than to communicate to him a large variety of particular applications. It is only just to state, however, that in Professor Wright's book the applied problems constitute a particularly valuable feature.

In the first chapter a certain lack of precision in language makes itself felt more than in the rest of the work. This laxness may be due to an effort to be "practical" and to avoid mathematical forms of expression. But a student who has mastered as much trigonometry, analytic geometry, and calculus as is necessary for reading the book intelligently, will appreciate accuracy and profit by it. He will have no difficulty in perceiving that we can speak only of the path of a point or particle, but not (without special definition) of "the path of a body." He will readily understand the application of the terms "constant" and "variable" to a velocity as soon as he knows how velocity is measured. "Uniform motion" should have been defined; and "constant velocity" would be preferable to "uniform velocity." In Art. 22, p. 18, line 5, the words "with constant velocity" should be omitted; in Art. 23, p. 19, read "arithmetically" twice instead of "algebraically;" in Art. 31, p. 26, for "polar axis" read "radius vector." A more serious confusion of ideas occurs in Art. 30, p. 25. Here d^2s/dt^2 is treated as if it represented the total acceleration of a point moving in a curve, and the rectangular components of the acceleration are derived in an erroneous way. This confusion could hardly have arisen if the general expressions for tangential and normal acceleration were derived; but these important expressions are not given. In the introduction the author states his preference for the term "Phoronomics," instead of "Kinematics," although he uses generally the latter term. What he had in mind was probably the term "Phoronomy," advocated by W. H. Besant (see *Nature*, Vol. 45, p. 462, and the preface to the second edition of Besant's Dynamics, 1893) and used by a number of other writers (Hermann, Kant, Budde, etc.).

In his discussion of Newton's laws of motion, the author takes a less critical and independent position than he did in the first edition. He deals with these laws by the philological method of interpreting a text, or rather of interpreting various meanings into the text, not by the scientific method of free investigation. The interpretation of the first law appears particularly strained. Thus, our author says (p. 45): "The law lies beyond our experience, as we have no experience of one body not acted upon by another." Have we any experience of one body acted upon by another? Is not all such action pure hypothesis? "Our direct experience goes, however, a certain distance in confirmation of the law * * *," and then he proceeds to show that observation and experiment tend to confirm the law. But the statement that a particle does not change its state of rest or motion without some cause is merely a truism; for where there is no effect there is no need for inventing a cause; and the statement that any change in motion is brought about by force merely supplies a name for the cause, at least as long as force is not otherwise known and defined. Newton's first law is, therefore, not the expression of an experimental fact; it merely assigns the name force to the cause producing change of motion (more exactly, of momentum). On the same page the author says: "From the law we learn that rest and motion are equally states of a body, the body being wholly without influence on its rest or motion. This property of matter is called *inertia* * * *." That the body, *i. e.*, the mass of the body, is wholly without influence on its rest or motion is hardly correct; otherwise, why should we take the mass of the body into account in mechanics? What the author means is, of course, that matter does not change its motion by itself, *i. e.*, without some cause. But is not this a truism? It must, however, be admitted that it is by no means an easy task to give a fully satisfactory presentation of the fundamental laws of motion suited to an elementary book.

Professor Wright has evidently made a careful study of the question of standards and units; his work contains a large amount of valuable information on this subject. In the matter of "weight" and the units of mass and force he follows Professor Greenhill; from page 57 on, all the m's are changed into w/g. Gravitation units are used throughout, the "metric units" being reserved for the final chapter. Considering the elementary character of the work, this is perhaps the best plan. But the rapid advance of electrical engineering may perhaps be counted upon to bring the absolute units into better repute among the engineers of the future.

Finally, attention might be called to the excellent articles (96–100) on the motion of projectiles which probably also owe something to Professor Greenhill.

ALEXANDER ZIWET.

Theory of Physics. By JOSEPH S. AMES, Ph.D. 8vo, pp. xviii + 513. New York, Harper & Brother, 1897.

The reading of this book was undertaken with the lively interest that one has in a new book upon one's own subject coming from a leader in one of the foremost institutions. To introduce a student into the domain of Physics is no mean task, and he who can furnish the clearest and most stimulating introduction has done much toward promoting the growth and development of the science. Here as elsewhere it can be said that "Well begun is half done."

In the reading of the text, it has been kept in mind that the book presupposes no previous training on the part of the student or at most a very elementary one. It is divided as follows: Book I., Mechanics and Properties of Matter; Book II., Sound; Book III., Heat; Book IV., Electricity and Magnetism; Book V., Light.

The chapter on kinematics treats motions of translation and rotation in a somewhat general way, makes a sharp distinction between velocity, a directed quantity, and speed, a mere number, which seems rather difficult to maintain, for the author on page 24 speaks of an acceleration, or directed quantity, as being opposed to a speed, and touches lightly upon harmonic motions and the spinning top. Harmonic motion hardly receives the share of attention its importance in sound and light seems to demand.

Dynamics is devoted to the consideration of matter in translation, rotation and equilibrium, to work, energy and mechanics, and to a short but valuable treatment of wave-motion. A marked feature of the style of treatment is the tendency to begin with a generalization and deduce from it the special cases, as the "Principle of the Conservation of Momentum" is taken for the starting point in the consideration of matter in translation, and the "Principle of the Conservation of the Moment of Momentum" for matter in rotation. From a logical point of view, this leaves nothing to be desired, but many teachers will question whether this is the easiest and surest way for an immature student to approach the subject for the first time. The successful effort made to connect the formulæ for matter in rotation with the corresponding ones for translation must, however, be helpful to the student.

The principle of stable equilibrium has received a very wide application in the book and illustrates again the author's fondness for applying general principles to particular cases, as it is applied to dissociation, evaporation, expansion, fusion and solution. Book I. is completed by a short chapter on gravitation and one on "Properties of Size and Shape of Matter" in which several formulæ are introduced "which may be deduced by the help of the higher mathematics."

Sound is defined as a sensation and the theory of sound in physics as the study of the *nature* of this *sensation* and the exact conditions under which it is produced. After defining sound as a sensation, it must be a little confusing to the careful student to read, "if the velocity of sound in any gas is known, etc." Light also is defined as a pure sensation, yet we read "if red light had a greater velocity than blue, etc." It is certainly a question whether physicists ought to surrender these useful terms which have long stood for the physical bases of the corresponding sensations and for whose use in this sense we have the sanction of the standard dictionaries, and substitute for them the awkward circumlocutions that would be necessary. It ought to be mentioned that Döppler's principle is given correctly as it is not in several current books.

Book III. Heat furnishes a clear and logical presentation of the study of the "so-called heat-effects," and closes with a short but good elementary treatment of the kinetic gas-theory and a two-page chapter on thermodynamics which will hardly succeed in giving the student a conception of the second law of thermodynamics.

Electricity, magnetism and light are treated almost entirely from a theoretical standpoint and here the title of the book finds its strongest *raison d'être*, though the character of the whole book is true to the purpose indicated in the title, "Theory of Physics." Much is made of lines of force and the energy of the dielectric and Poynting's theory of the flow of energy perpendicularly into a conductor is more than suggested; the dissociation theory of electrolysis is given at some length and applied to the explanation of conduction in solids.

Light is treated entirely from the side of physical optics and the principle of curvature of the wave-surfaces is used throughout the treatment of reflection and refraction. There is no question of the elegance of this method nor of the insight into wave propagation furnished by a clear understanding of it, but that it lends itself to an elementary presentation of the subject of light is somewhat doubtful. The idea of curvature is not easy for a student to grasp, and the constructions of images and the tracing of the path of the light is not so easy from this point of view, and it will be noticed that the author has drawn many diagrams in which rays are really used, though nothing is said of the change from wave to ray construction. It is trusting a great deal to the student's power of constructive imagination to reduce a lens to a straight line, as is done in most of the diagrams. This is only an apparent simplifying of the diagrams.

Throughout the book statements are made very guardedly, and few are clear cut and positive—such statements as students and many teachers like to find. We read under gravitation that "there *seems* to be undoubted evidence that each portion of matter in the universe influences the motion of every other portion," and in reference to a helix we find that "when this form of a conductor carries a current, it is *sometimes* called a solenoid" and "a solenoid with a soft iron bar inside is *sometimes* called an electromagnet." In striking contrast to these statements is the frequent use of the phrase "of course," when it is not a matter of course at all. Few errors in the text were noticed, and these are mainly typographic; on page 103, "a piece of cork is a very rigid body;" page 238, "If the temperature of the lead is not very *hot*," and the lettering of figures 3 and 232 does not agree with the text.

The omission of problems that illustrate and tend to fix the principles enunciated in the text will be felt by many teachers; however, this gives to each teacher the chance to select problems in accordance with his own ideas of the relative importance of the principles.

Chas. E. St. John.

The Old Light and the New. By William Ackroyd, F.I.C. 12mo, pp. 102. London, Chapman & Hall, 1896.

Roentgen Rays and Phenomena of the Anode and Cathode. By Edward P. Thompson, M.E., E.E., etc., and Prof. William A. Anthony. 8vo, pp. xiv + 190. New York, D. Van Nostrand Company, 1896.

The X-Ray; or, Photography of the Invisible. By William J. Morton, M. D., and Edwin M. Hammer. 8vo, pp. 196 + 32 half tone plates. New York, The American Technical Book Company, 1896.

Les Rayons X et la Photographie de l'Invisible. By Georges Vitoux. 12mo, pp. 191. Paris, Chamuel, 1896.

It is interesting to compare these four little books which were brought forth, almost simultaneously, to meet the popular demand for X-ray literature. Those who take up Mr. Ackroyd's book in the expectation of finding therein any adequate discussion of the Roentgen phenomena will be disappointed. The volume is in fact devoted chiefly to an attempt to extend, by the aid of very inadequate data, the author's theory concerning the relations of color to chemical composition to the explanation of the varying opacities of bodies to the X-rays.

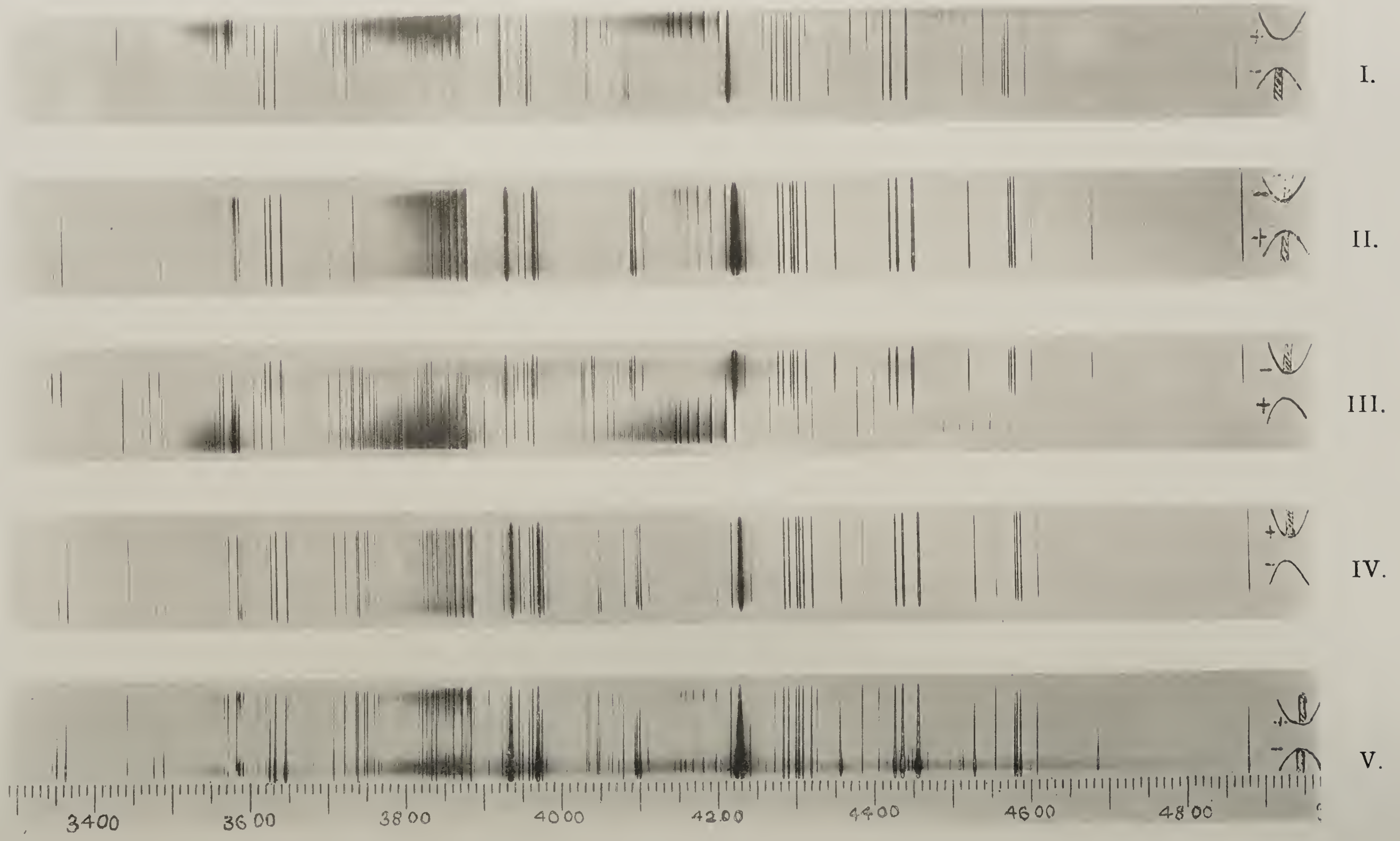
The other three volumes under consideration differ widely in their treatment of the subject. Messrs. Thompson and Anthony devote themselves chiefly to the compilation of a very useful and complete summary of the rapidly growing X-ray literature up to the time that their book went to press. The chief fault of their work may be said to be the lack of arrangement; but indeed, it is difficult to imagine in what way the heterogeneous mass of observations which were published during the first months after the announcement of Röntgen's discovery could have been arranged. The book owes its usefulness chiefly to the fact that it gives brief and intelligible abstracts of the numerous papers which had been published and that reference is made in every case to the originals. The authors deserve the thanks of the scientific public, likewise, for refraining to reiterate in their book the familiar electrical principles upon which production of the X-ray depends.

He who opens the volume of Morton and Hammer at its first page will think that he has happened by mistake upon one of those saddening *primers of electricity* with which the book market has been flooded of recent years. Nor will he be disabused of his mistake until nearly sixty pages have been perused. If, however, he perseveres until he reaches Part III., entitled "Operation," he will have his reward. From this point onward the book takes on a useful form. The description of the arrangement of apparatus for X-ray photography is definite, and impresses one at once as the work of those who write out of their own experience. The chapter treating of the relation of the vacuum to the production of X-ray tubes containing, as it does, a detailed description of methods of both increasing and diminishing the vacuum by artificial means, is especially useful.

By far the most readable of the books under consideration is the treatise of Georges Vitoux. The author approaches the subject in a systematic manner. He assumes a reasonable elementary knowledge on the part of his readers, and confines himself to a well-arranged historical review of his subject. An adequate idea of the contents of this interesting little volume can be best given by taking it up chapter by chapter.

The author first describes the discovery of the X-rays, giving a vivid account of the popular excitement produced by Roentgen's announcement. He devotes his second chapter to the translation of Roentgen's original paper into French, and then proceeds to a discussion of the results obtained by earlier workers in this field. It may be questioned whether it was wise to include in this list the unsuccessful attempts of Reichenbach to demonstrate by means of the daguerreotype the existence of the *odylic effluvia;* but with this exception, the historical sketch is admirable. It is followed by chapters upon the nature and origin of the X-rays, their properties and their relation to fluorescence. The remainder of the volume is devoted to the technique of operation and to the applications of X-ray phenomena. It is a question whether the brief and non-commital statements concerning "*black light*" photography in the dark, and by electricity, and the so-called "*photography of thought,*" might not have been omitted to advantage.

E. L. Nichols.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ELECTRICAL ENGINEERING DEPARTMENT
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PHYSICAL REVIEW.

ARC SPECTRA.

By Arthur L. Foley.

WHEN the image of a normal electric arc is examined it is seen, as regards color, to consist of three regions. The central portion, 1, Fig. 1, is violet. At times it extends entirely across from the positive to the negative carbon, as in Fig. 1. At other times it appears at the positive carbon only, as in Fig. 2. It is always broadest at the positive carbon.

Surrounding the violet center is a sheath of blue (2), which frequently extends entirely across the arc at the negative carbon, as in Fig. 2, but never at the positive carbon. The outside sheath (3) is yellow. It is narrow or thin at the lower carbon and broad at the upper carbon, the lower end of which it completely surrounds.

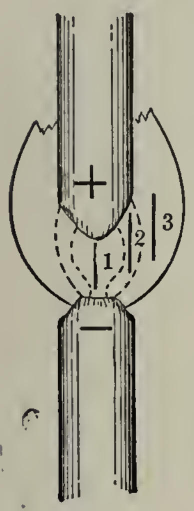

Fig. 1.

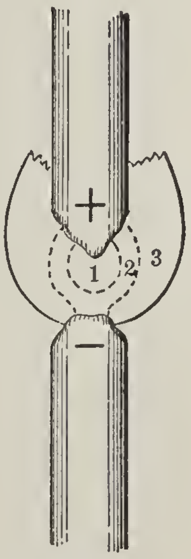

Fig. 2.

A reversal of the direction of the currents shows that sheaths (1) and (2) are quite definite in position as regards the positive and negative carbons. The flame like character of (3) is shown by the fact that it is practically independent of the direction of the current.

These different sheaths, which present such a contrast in color, were examined spectroscopically by Lockyer[1] in 1879. Prior to that time Lockyer and others had worked with the spectrum of the arc maintained by a current from a battery. To minimize trouble and expense, it was customary to reduce the number of battery cells required by burning a short arc. Indeed the image of the arc was so short that in order to obtain lines of any length it was necessary to focus the image of a horizontal arc upon the vertical slit of a spectroscope. Under such conditions the regions before mentioned scarcely appear.

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[1] Proc. Roy. Soc., XVIII., p. 425, 1879.

When Lockyer was enabled to substitute a dynamo for his battery, he began to use a long vertical arc and a vertical slit. He focussed different parts of the arc upon the slit of a spectroscope, and obtained photographs of the spectra. He remarked upon the theoretical importance of the phenomena observed, and upon the great complexity of the various flame sheaths, especially when certain foreign substances were introduced into the arc. His results are interesting, but not very definite. He found that, in the ordinary arc, the carbon flutings clung to one pole and the calcium lines to the other. But when calcium was introduced into the arc, "the blue calcium lines were visible alone at one pole and the H and K lines, without the blue, at the other." He found a progression of lines from pole to pole. "They lie en échelon along the spectrum." Some lines, while extending entirely across the spectrum, were irregularly thickened at different levels of the arc. A somewhat analogous result has been found in the spectra of gas flames at different levels.[1]

Miss C. W. Baldwin[2] recently investigated the subject with somewhat surprising results. She found that the carbon and many of the metallic lines were absent or greatly weakened in the outer sheaths, *while certain other lines, those most brilliant near the negative carbon, were strengthened.* The introduction of potassium, sodium, lithium, barium, calcium, and strontium changed the characteristic band spectrum of the arc by weakening those lines which were strongest at the positive carbon, while copper and silver had but little effect upon the original spectrum. Zinc and cadmium appeared to affect most the lines near the negative carbon.

The present research was undertaken for the purpose of verifying the results of previous investigations on this subject, and of so extending the work as to make it possible to arrive at some more definite conclusion.

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[1] The Spectra of Metals at the Base of Flames. M. Gouy, Phil. Mag., Vol. III., p. 238, 1879.

[2] A Photographic Study of Arc Spectra. C. W. Baldwin, Physical Review, Vol. III., pp. 370 and 448, 1896.

Apparatus.

A Rowland concave grating of 6½ feet radius and 14,438 lines to the inch, was placed upon a Brashear mounting, Fig. 3. The grating was the choice of three, selected because it gave a uniformly bright first spectrum, and very weak spectra of higher orders.

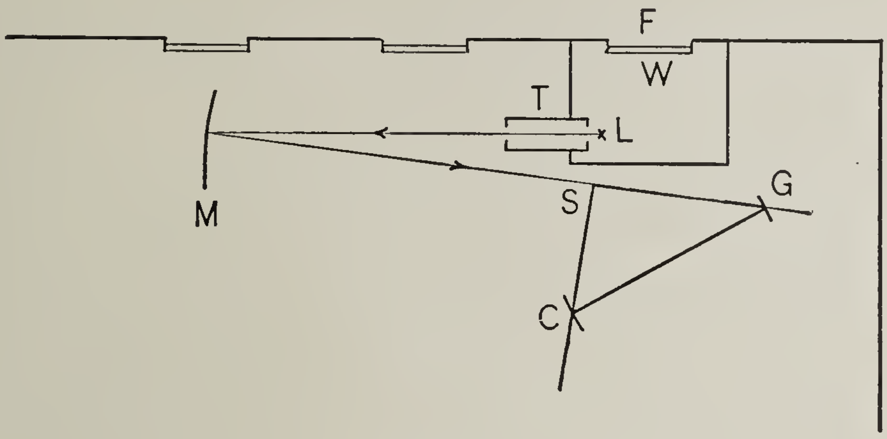

Fig. 3.

The slit *S* had the usual adjustments for width, length, and direction. The width generally used was 0.02 mm. The light from an arc lamp *L* was focussed upon the slit by a concave mirror *M*, of ten inch aperture and twelve feet radius. The mirror, being somewhat scratched and tarnished, was resilvered by the Brashear process as described by Wadsworth.[1] The lamp, which was in the same horizontal plane with the mirror, slit and grating, was placed inside a small closet built out in the room, and as near the slit as possible. The image was the same size as the arc, and as it lay near the axis of the mirror, the distortion was reduced to a minimum. A tube *T*, with a diaphragm at each end, served to cut off superfluous light. However, it was found impracticable to screen off all of it. The apparatus was set up in a large room with a number of windows *W* and other openings. It was decided to enclose the slit, grating, and camera, and to make no attempt to darken the room.

The first arrangement consisted of a number of metal rings placed crosswise at intervals along the track *SG* and the carriage arm *CG*. Those on *SG* were arranged to slide along the track so as to permit of the carriage arm being moved into any position desired. About these rings was wrapped black cotton flannel, in such a way as to form two hollow cylinders completely enclosing the camera and grating and all of *S* but the slit itself. This arrangement was not satisfactory. When photographing the red end of the spectrum, which required a comparatively long exposure, the violet portion of the first spectrum together with the spectra of higher orders, fell upon the walls of the tube between the grating and camera, and although the nap of the cloth was roughed up and turned inside, it was found that there was sufficient reflected light to fog the plate. The arrangement was modified by removing the cloth and rings along *CG*, and substituting for the cloth cylinder a long tapering box, Fig. 4, somewhat larger than the camera box at the end *C*, and about the size of the grating holder at the end *G*. A portion of the side of the box at the end *G* and next *SG* was moved to allow the light from the slit to reach the grating. Along the inside walls of the box were tacked strips or vanes of tin in the manner shown in the figure, and the whole interior was painted dead black. A piece of cloth at each end served to make the apparatus light tight. This arrangement was entirely satisfactory. An exposure of thirty minutes failed to show any fogging of the plate due to light reflected from the walls of the box, or to the light of the room. To move the carriage arm into a new position it was necessary only to adjust the length of the cloth tube along *SG*.

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[1] Astrophysical Journal, I., p. 252, 1895.

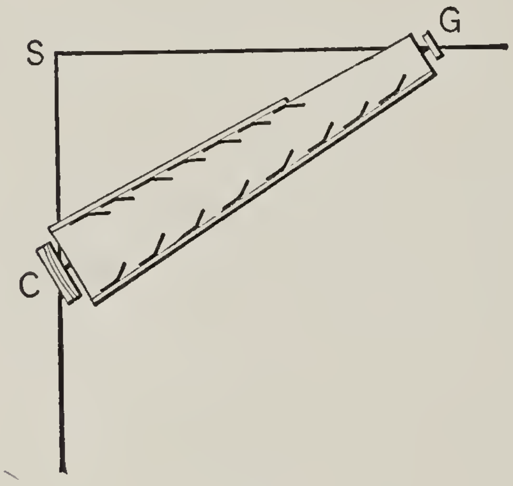

Fig. 4.

The camera box provided with the mounting was arranged for 1½ x 5 inch straight glass plates. This box was discarded, and one was constructed for 2 x 8 inch plates, curved to 3¼ feet radius to conform to the grating. Celluloid films were tried, but they were found to be unsatisfactory where accurate measurements were necessary. The thinnest glass plates that could be obtained (three dry plate companies were tried) invariably broke when an attempt was made to bend them to the proper curvature. It was noticed that they did not break when put in backwards; that is, with the film on the back or convex side. This suggested the plan of coating both sides, or of pasting on the back of the plate something that would act the part of the film. Rice paper was tried without success. The paper proved to be less tenacious than the film. The whole difficulty was overcome by changing the radius of curvature of the plate holder from 3¼ to 6½ feet. The plates were easily bent to the latter curvature without breaking. That the spectrum remained a normal one is shown as follows:

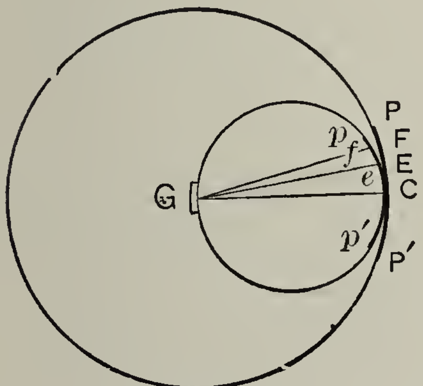

Fig. 5.

Let *G*, Fig. 5, be a concave grating of radius *GC*. With *GC* as diameter construct a circle. With *G* as center and *GC* as radius construct a second circle. The two circles are tangent at *C*.

All the spectra from the grating (supposed mounted as before described) are in focus on the first circle; and the usual method is to bend the plate *pCp′*, into an arc of this circle. But suppose the plate *PCP′* lies in the circumference of the second circle, and consider only the rays of light that proceed from the center of the grating, as *GC*, *GE*, etc. As the arc *Ce* measures the angle *GCe* at the circumference of the first circle, and the arc *CE* measures the same angle at the center of the second, the arc $Ce =$ the arc *CE*. Likewise $fe = FE$, so that equal lengths on the plate *PCP′* represent equal differences in wave-length, as they do on the plate *pCp′*. The plate *PCP′* is not accurately in focus except at *C*; but unless one uses longer plates than is usual in such work, the definition is practically uniform. The lines at the ends of the eight-inch plates used in this investigation appeared just as sharp and clear under the microscope, as did those at the center. Even if sufficiently thin glass plates can be secured, the fact that such plates are seldom plane, and that they warp, and bend very unevenly, suggests the advisability of reducing the curvature of the plate when a short focus grating is used.

I. *The Spectrum of the Ordinary Arc.*

This study was made under four conditions : (1) a vertical arc and a vertical slit, (2) a vertical arc blown out by a horseshoe magnet, (3) a horizontal arc and a vertical slit, (4) a horizontal arc and a horizontal slit.

1. *Vertical arc and vertical slit.*—By means of a rack attached to the upper carbon holder of a Thomson-Houston arc lamp, and a pinion which passed through the wall of the closet and was operated by a knob near the slit, the length of the arc was kept equal to or greater than the length of the slit—about 1.5 cm. When the arc happened to break during an exposure the slit was covered until the carbons were brought together and the arc drawn out as before. In this way photographs were obtained free from the continuous spectrum of the hot carbon points.

Photographs were taken with the slit in a dozen or more positions. Tables I, II and III give the results of a study of the three regions of the arc with the slit vertical and through the center of each region. Two exposures were made in each position, three seconds and thirty seconds in the violet and blue regions, ten seconds and thirty seconds in the outer or yellow sheath. An exposure of three seconds in the latter brought out only ten lines.

The tables extended from $\varphi=3092$ to $\varphi=5015$. The measurements were made with the plate on the bed of a dividing engine, and the lines were identified by reference to the tables of Kayser und Runge,[1] Rowland,[2] and Hasselberg.[3]

On each plate there were traces of lines too hazy or indefinite to be accurately measured. A few lines on Plate 2 were so blurred by over-exposure that their measurements were doubtful. With these exceptions every line on each of the six plates was carefully measured. The accuracy of the measurements was sufficient for the identification of the lines in almost every case. The intensity of each line was estimated at three points: at the upper end or near the upper or positive carbon, at the center, and near the lower end or negative carbon. In Table I the lines are classified as to

[1] Kayser und Runge, Ueber die Spectren der Elemente.

[2] Astrophysical Journal, I., p. 29, 1895.

[3] Astrophysical Journal, IV., p. 116, 1896.

Table I.

Elements.	Region 1. Central Violet.		Region 2. Blue.		Region 3. Outer Yellow.	
	Plate 1. Exposure 3 Seconds.	Plate 2. Exposure 30 Seconds.	Plate 3. 3 Seconds.	Plate 4. 30 Seconds.	Plate 5. 10 Seconds.	Plate 6. 30 Seconds.
C	313	450	60	274	1	8
Fe	84	107	29	91	47	74
Ca	14	17	3	15	8	15
Ti	0	20	0	1	0	1
K	3	4	2	3	2	3
Al	2	3	2	2	2	2
Mn	2	2	0	2	1	1
Cu	0	2	0	0	0	0
Ba	1	2	0	1	1	1
Na	1	1	0	1	0	1
Si	1	1	1	1	0	1
Not identified	4	50	0	1	0	0
Faint or blurred . .	29	37	38	29	6	13
To. No. lines identified .	421	709	97	391	62	107
To. No. lines visible . .	454	796	135	421	68	120

Whole number of lines visible, 1994.
Whole number of lines measured, 1842.
Whole number of lines identified, 1787.

number and source. The table shows that the lines rapidly decrease in number as the slit is moved from the center of the arc to the outer edge. This is due chiefly to the fading out of the carbon bands, only a few of the stronger carbon heads being visible in the outer sheath. But the fading out is not confined to the carbon, as the lines of all the other elements of the table show the same tendency, though to a much smaller degree. This fact is better brought out in Table II., where the general character of the lines is indicated. The estimates given for any element represent as nearly as possible the average of all the lines of this element, in the given position. The first estimate is for the upper end of the line, near the positive carbon ; the second for the center ; the third for the lower end, near the negative carbon. There is a decrease not only in the *number* of

Table II.

Element.	Region 1. Central Violet.	Region 2. Blue.	Region 3. Outer Yellow.
C	Strong	Weak	Trace
	Fairly strong	Trace	Absent
	Weak	Absent	Absent
Fe	Strong	Fairly strong	Weak
	Strong	Fairly strong	Fairly strong
	Strong	Fairly strong	Weak
Ca	Strong	Strong	Fairly strong
	Strong	Strong	Strong
	Very strong	Very strong	Fairly strong
Ti	Absent	Absent	Absent
	Very Weak	Trace	Trace
	Absent	Absent	Absent
K	Weak	Weak	Very weak
	Weak	Weak	Very weak
	Strong	Fairly strong	Weak
Al	Fairly strong	Weak	Weak
	Strong	Fairly strong	Weak
	Strong	Fairly strong	Weak
Mn	Weak	Very weak	Trace
	Weak	Very weak	Absent
	Very weak	Very weak	Absent
Cu	Very weak	Absent	Absent
	Very weak	Absent	Absent
	Very weak	Absent	Absent
Ba	Fairly strong	Fairly strong	Weak
	Fairly strong	Fairly strong	Weak
	Strong	Fairly strong	Weak
Na	Fairly strong	Absent	Absent
	Fairly strong	Trace	Trace
	Strong	Trace	Trace
Si	Strong	Fairly strong	Trace
	Fairly strong	Weak	Absent
	Weak	Trace	Trace

lines as the slit is moved outwards from the center of the arc, there is a corresponding decrease in the average *intensity*. The two tables are in complete agreement, for the intensities would be expected to decrease as the number of lines diminishes.

On pages 376 and 378 of Miss Baldwin's paper, previously referred to, a number of lines are given with the statement that they appear to be stronger in the outer regions of the arc than in the central region. To test this matter Plates 2, 4, and 6, which were exposed equal times (30 seconds) and developed under the same conditions, were placed side by side on the carriage bed of a dividing engine. They were adjusted so that the microscope, sliding in its track at right angles to the bed of the engine, could be brought successively over the same line on each. Any other line could then be brought under the microscope by moving the carriage.

Table III.

Wave-length.		Region 1. Central Violet.	Region 2. Blue sheath.	Region 3. Outer Yellow.
3624.15	Ca			
3630.82	Ca	Weak		
3644.45	Ca	Weak		
3706.18	Ca	Weak	Weak	Very weak
3737.08	Ca	Fairly strong	Fairly strong	Fairly strong
3812.20	C	Fairly strong	Weak	
3816.02	Fe	Fairly strong	Fairly strong	Weak
3820.59	Fe-C	Strong	Strong	Fairly strong
3827.04	Fe			
3832.46	Mg			
3834.42	Fe	Fairly strong	Fairly strong	Fairly strong
3838.44	Mg			
3841.24	Fe-Mn	Fairly strong	Weak	Weak
3856.54	Fe	Fairly strong	Fairly strong	Fairly strong
3860.05	Fe-C	Strong	Fairly strong	Fairly strong
3933.83	Ca	Very strong	Very strong	Strong
3960.05				
3968.63	Ca	Very strong	Very strong	Strong
4076.96	Fe			
4226.91	Ca	Very strong	Very strong	Very strong
4299.14	Ca	Fairly strong	Fairly strong	Weak
4302.68	Ca	Strong	Fairly strong	Fairly strong
4318.80	Ca	Strong	Fairly strong	Fairly strong

This arrangement was advantageous, as a comparative study could be made of the same line on each of the three plates, at the same time. Table III gives the results of the study. A blank space indicates either that the line was absent, or that it was too hazy or too blurred to be measured.

The tables show that the yellow or flame portion of the arc was composed chiefly of the vapors of calcium and iron. The lines of these elements were relatively much stronger in the outer regions, than the lines of any other element. All the lines of any one element did not fade out at the same rate. Most of those of Table III were relatively much stronger in the outer regions of the arc than were the other lines due to the same element. But it does not appear that there are any lines which have a maximum of intensity in the outer sheaths. The opposite conclusion may have been brought about by failing to consider some of the following points :

a) Time of exposure. Table I shows that an exposure of three seconds in the violet region brings out almost four times as many lines as an exposure of thirty seconds in the yellow sheath, so one is inclined to expose a plate much longer in the outer region. Miss Baldwin[1] says that this was done in her investigation. If the absolute intensities of the lines are in question, the arc must be maintained as nearly constant as possible, and the exposures must be equally timed.

b) Time of development. When an exposure is made in the violet region, the carbon (or cyanogen) bands (heading near $\lambda = 3883$, $\lambda = 4216$, and $\lambda = 3590$) appear almost immediately after the plate is placed in the developer. Unless the exposure has been very brief a prolonged development causes the carbon lines near the several heads, especially on the side toward the shorter wave-lengths, to run together to such an extent that their identity is lost. So the tendency is to underdevelop. The reverse is the case in the outer sheaths of the arc where the carbon bands are weak or absent, and the metallic lines are not so numerous. When intensities are to be compared the plates must be developed under the same conditions. An underdevelopment frequently brings out differences of intensities that do not appear on a fully developed plate. The differences are usually better brought out in an underexposed plate which has been fully developed.

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[1] Physical Review, Vol. III., No. 17, p. 375.

c) The tendency is to overestimate the intensities of the lines of the outer sheath, as they stand out prominently on a clear background, while many of the lines of the violet region are more or less masked by the superposed carbon spectrum.

d) Variations in the intensities of the lines due to accidental causes (fluctuations of the lamp, lack of uniformity of the carbons, etc.) must be eliminated by extending the study to a large number of plates.

In the present investigation a metronome was used to time the exposures. All the plates of any one series were placed in the same developing bath at the same time and were allowed to remain the same length of time. An amidol developer was used. Fifty-six plates were studied in this manner.

That there are no lines which have a maximum of intensity in the outer sheaths of the arc is not proven by the work already described. For, when the central or violet region was focussed on the slit, the latter received light from all the other sheaths also, and the spectrum obtained consisted of the superposed spectra of all the sheaths. In position (2) (Fig. 1) the slit received light from sheaths (2) and (3). In position (3) only was the light confined to a single sheath.

This objection loses a part of its force when one considers that a greater thickness of the outer sheaths was presented in positions (2) and (3), than in position (1), where the sheaths were normal to the slit, and that the light from the central and hottest portion of the arc was somewhat diminished by the absorption of the outer and cooler portions.

2. *Vertical arc blown out by magnet.*—When a vertical arc is blown sidewise by a horseshoe magnet, Fig. 6, the central region is but slightly displaced. The blue region is considerably thicker on the side opposite the magnet. The yellow region is almost entirely on that side. If now the slit is placed in position (1) it receives but little light from the thin outer sheaths.

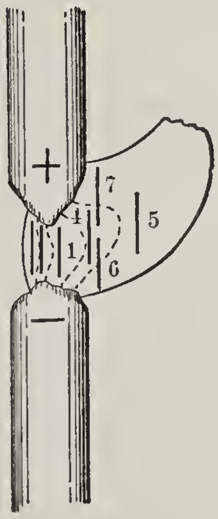

Fig. 6.

Photographs were taken with the slit in the seven positions indicated. No lines having maxima in the outer sheaths were found. If there were any such lines, it might be expected that an exposure in position (5) where there is a great mass and depth of flame would show stronger lines than an exposurein position (3). Such was not the case. There were lines at (3), also at (6) and (7), not found at (5).

When the slit was shortened until it extended but a fraction of the distance between the carbon points, lines were found at either (6) or (7) which were weak or absent at (1). When the slit was lengthened until it extended from pole to pole, no such lines were found. The explanation is not far to seek. The tendency of the carbon lines is to cling to the positive pole (Table II.) while most of the other lines are strongest at the negative pole. Consequently in position (7) we would expect some carbon lines not present at (5) or (6), and at (6) some metallic lines which are weak or absent at (5) and (7). A very short slit between the poles in position (1) does not give the true intensities of those lines which tend to cling to the carbons; many of these lines do not appear at all. But when the slit is lengthened until it extends from pole to pole, *all the lines of the arc are found to have a maximum of intensity in the central violet region.*

3. *Horizontal arc and vertical slit.*—When the arc was horizontal the atmospheric currents were so strong that it was not necessary to bring the magnet very near the arc to blow it out, as shown in Fig. 7. The current through the lamp was maintained at a much higher voltage than when the arc was vertical. Photographs were taken with the slit in three positions: (1) half-way between the carbons, (2) near the positive carbon, (3) near the negative carbon. As the slit extended almost across the arc the spectra of all the regions was obtained on each photograph, making a comparative study easy. All the lines were strongest in the violet region. The carbon lines were stronger in position (2) than in positions (1) or (3). The metallic lines averaged a trifle stronger near the negative carbon.

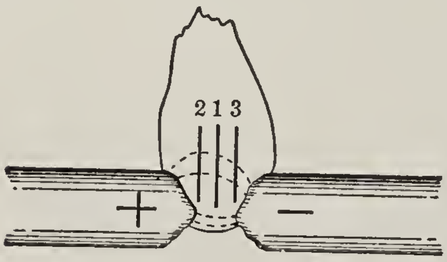

Fig. 7.

4. *Horizontal arc and horizontal slit.*—The arc was maintained as

before. The ends of the carriage beam of the Brashear mounting (Fig. 3), were firmly clamped to the track. The grating holder was modified so as to hold the grating in any position. The entire arrangement was then turned on its edge, so that the plane of the tracks was vertical, and the slit horizontal. Photographs were taken in three positions (Fig. 8). The results agreed with those obtained by the previous methods.

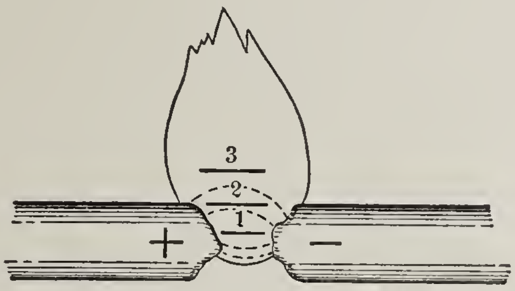

Fig. 8.

Neglecting for the present the thickening of the lines at the poles and considering each region of the arc as a whole, it may be concluded that the differences observed in the spectra are due chiefly, if not entirely, to temperature differences. This view was rejected by Miss Baldwin because the lines of those metals which have the highest melting points were not strongest in the hottest part of the arc. K, Na, and Li were strengthened at the negative carbon; so also were Ba, Sr, and Ca, which have relatively high melting points.

It will be shown in the second part of this paper that the strengthening of the metallic lines at the negative carbon is due to the electrolytic nature of the arc. Though the temperature of the negative carbon is considerably below that of the positive in the ordinary arc, usually the difference is not so great when a metallic salt is introduced into both carbons. Moreover, the temperature of the negative carbon is sufficiently high to vaporize rapidly the elements mentioned.

A careful examination of the image of the arc suggests that the colors of the three apparently well-defined regions are due to temperature differences. The violet center is surrounded by a blue sheath, but the change from the violet to the blue is gradual. The outer yellow sheath is green at its inner edge, and orange-red at the outer. In short, there is in a way a color progression, apparently due to a fall of temperature from the center of the arc outwards.

It was noted in the first part of this paper that the violet center sometimes extends to the negative carbon, sometimes not. It does so when the current is strong and the carbons are very hot. As the current is decreased the temperature of the carbons falls, and the violet region contracts about the end of the positive pole, finally disappearing entirely.

II. *Spectra of the Elements.*

The spectra of twelve elements were studied to determine the nature of the lines near the carbons and directly between them. The spectrum was obtained by removing the core of *one* or *both* of the carbons and replacing it by the salt of the metal to be studied. The salts used were barium carbonate, sodium nitrate, the chlorides of zinc, calcium, strontium, potassium and lithium, the sulphates of chromium, cadmium and aluminum, and the oxides of rubidium and titanium.

Calcium and strontium were studied with both the vertical and the horizontal arc, under the four conditions described in part I of this paper. With the exception of the thickening of the lines at the poles, the differences in the spectra obtained from various parts of the arc could be attributed to temperature differences. Therefore, the study of the remaining elements was limited to the violet region of the vertical arc, with a vertical slit extending from pole to pole.

Six photographs were taken of the spectrum of each element. The conditions were as follows :

No. I., upper carbon plain (containing no metallic salt) and positive, salt in lower carbon.
No. II., upper carbon plain and negative, salt in lower carbon.
No. III., salt in upper negative carbon, lower carbon plain.
No. IV., salt in upper positive carbon, lower carbon plain.
No. V., salt in both carbons, upper carbon positive.
No. VI., salt in both carbons, lower carbon positive.

The accompanying plate gives five of the six photographs in the case of calcium chloride. The markings at the right end of each spectrum are intended to show at a glance the direction of the current and which of the carbons contained the salt.

The discussion of this part of the subject can be shortened by stating in advance some of the conclusions arrived at, as follows :

The arc is electrolytic. The electropositive elements seek the negative pole, and the electronegative the positive pole. Hence the thickening of the metallic lines at the negative carbon. Convection currents due to heated gases in the arc may be sufficiently strong to mask the true nature of the lines.

No. V. (see plate), in which the core of each carbon is filled with calcium chloride, shows the thickening of the lines at the negative pole. The lines at the positive pole are not much stronger than in No. I., in which the lower carbon only contains the salt. In the latter the lines at the positive pole are due to the calcium vapors formed at the negative carbon, and carried upward by heat convection currents, for, in No. III., in which the negative carbon, still containing the salt, is placed above, almost all of the *calcium lines appear at the negative pole only.* The exceptions which may be seen in the photograph are due to the calcium contained as an impurity in the carbons. They were just as strong when both carbons were plain. No. IV., shows that convection currents alone do not bring about the conditions of No. III. In No. IV. the tendency of the lines to seek the negative carbon is sufficient to overcome the heat convection currents of the arc, and the lines are as strong at the lower carbon as at the upper.

All the lines given in Table IV. were visible on each of the negatives from which the accompanying plates were copied. Within the given limits the list embraces all but two of the lines catalogued by Kayser and Rungé. The absent ones are $\lambda = 4624.71$ and $\lambda = 4807.47$.

Table IV.

CALCIUM.

(Starred lines are present at both poles in Plate III, the others at the negative pole only.)

$\lambda = 3344.49$	$\lambda = 3949.09$	$\lambda = 4302.68$*	$\lambda = 4512.73$
3350.22	3957.23	4307.91*	4527.17
3361.92	3968.63*	4318.80*	4578.82
3468.68	3973.89	4355.41	4581.66
3474.98	4092.93	4425.61*	4586.12
3487.76	4095.25	4435.13*	4685.40
3624.15	4098.82	4435.86	4823.04
3630.82*	4226.91*	4454.97*	4833.85
3644.45	4240.58	4456.08*	4847.22
3706.18	4283.16*	4456.81	4878.34
3337.08	4289.51*	4508.04	5041.93
3933.83*	4299.14*	4509.89	

Of the forty-seven lines at the negative pole on plate No. III, only fourteen were visible at the positive carbon. They are starred in the table. As has been remarked before, these are the lines present in the arc spectrum between ordinary carbon points. Their intensity and number may be increased by increasing the time of exposure. But this increases the intensities of all the lines proportionately.

Ca photographs were chosen to illustrate the above effects because they represent about an average for the elements experimented upon. Ti did not show the effects at all. The lines extended across the spectrum with almost uniform intensity. Sometimes they were slightly intensified at the positive carbon, probably because of the higher temperature.

The Zn lines were slightly stronger at the negative carbon. The lines of Rb, K, Na, Li, Ba and Sr showed a much more decided preference for the negative pole than did the Ca lines. The preference was most marked in the case of Rb, though the most striking photographs were obtained with Ba, which has a large number of strong lines in this region of the spectrum.

As far as it could be determined the order of the elements, as regards the tendency of their spectral lines to cling to the negative carbon, is the same as their order in the electropositive-negative series. Table II. accords with this conclusion. The C, Si and Mn lines were found to be strongest at the positive pole, the Ca, K, Al, Ba and Na lines at the negative pole, while the Ti, Fe and Cu lines were about equally strong throughout.

The following experiment is more conclusive. The *negative* carbon of a horizontal arc was filled with calcium chloride. A *new plain* carbon formed the *positive* pole. A horizontal arc was used, otherwise the heat convection currents might have thrown doubt upon the results of the experiment. The slit was horizontal and extended from pole to pole. The carbons were placed 1 cm. apart and an arc was formed by passing between them a third carbon, which served to bring the poles in momentary contact. After one minute the current was shut off, and the carbon containing the Ca was replaced by a new plain carbon. The arc was then formed again by means of a third carbon, and the spectrum was photographed. *No Ca lines appeared except those always present in the ordinary arc.*

In the second part of the experiment a plain carbon was used as a *negative* pole, and the Ca was placed in the *positive* pole. The arc was formed as before, and allowed to continue for one minute. The positive pole containing the Ca was then replaced by a new plain carbon, the arc was formed and the spectrum photographed. *The Ca lines came out very clear and strong*, almost as strong as if the negative carbon had been *filled* with the salt. There can be but one conclusion. In the first case the Ca was in the negative pole and there was no tendency for it to pass over to the positive pole. In the second case it was placed in the positive pole, and it freely passed over to the plain negative pole. The latter became so impregnated with it that it was capable of giving a strong spectrum of Ca when it was afterwards used in an arc with a plain carbon.

Any of the elements, which show a marked tendency to cling to the negative pole, may be used instead of Ca in the above experiment. The result was doubtful in the case of Zn.

The electrolytic nature of the arc was further confirmed by a series of measurements of the voltage necessary to maintain an arc of given length between unlike electrodes. It was found that a much higher voltage was required when the metallic salt was placed in the negative pole than was necessary when the salt was in the positive pole. When one of the carbons was replaced by a metal rod, a higher voltage was required when the current was passing from the carbon to the metal, than when it was flowing in the opposite direction. The details of this portion of the work will be given in a subsequent paper.

III. *Spectrum of Carbon.*

A vast amount of work has been given to the investigation of the spectrum of carbon, and of carbon compounds. A greater amount remains to be done. The precise nature of the spectrum of carbon itself is still in question.

It has been noted before that the carbon or cyanogen bands, heading near $\lambda=3883$, $\lambda=4216$, and $\lambda=3590$, are the most prominent feature of the ordinary arc spectrum. The lines composing these bands are listed as carbon in Table I., though they are now usually attributed to cyanogen. They will be called the cyanogen bands, while by the carbon bands will be meant those heading near $\lambda=4737$ and $\lambda=4382$. There are other carbon and cyanogen bands but they are not included in this investigation except when specifically mentioned.

Ångström and Thalèn[1] attributed the cyanogen bands to a compound of carbon and nitrogen. Lockyer[2] found them to be as strong when the arc between carbon poles was in dry chlorine as when in air. Consequently he pronounced them carbon bands. Liveing and Dewar[3] questioned the purity of the chlorine and carbon used by Lockyer. They arranged an arc inside a glass globe into which was passed continuously a stream of the gas under experiment. The cyanogen bands were weak or absent in chlorine, hydrogen, carbon dioxide and carbon monoxide; they were strong in air, nitrogen and ammonia. That cyanogen was formed by the arc in air was proved by analyzing the contents of the globe.[4]

Lockyer[5] carefully removed the air from a tube containing carbon tetrachloride. When a spark, without a condenser, was passed between platinum terminals sealed into the tube, the cyanogen flutings were quite strong. They disappeared when a condenser was used, but in neither case did any hydrogen or nitrogen lines appear. Liveing and Dewar[6] repeated Lockyer's work using greater care to obtain pure gases. They failed to find the cyanogen bands in carbon tetrachloride. They studied the spark in several other compounds of carbon. The cyanogen bands appeared only when the compound was one that contained nitrogen. They were subsequently able to reverse the bands.[7]

Kayser and Runge[8] formed an arc between carbon terminals inside a block of retort carbon. When a stream of CO_2 was passed through the enclosure the cyanogen bands were weakened and the carbon bands strengthened. When air was introduced the former were strengthened and the latter weakened.

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[1] "Nova Acta Reg. Soc. Upsal.," Ser. III, vol. IX.

[2] Proc. Roy. Soc., 27, p. 308, 1878.

[3] Proc. Roy. Soc., 30, p. 152, 1880.

[4] Proc. Soc., 30, p. 85, 1880.

[5] Proc. Roy. Soc., 30, p. 335, 1880.

[6] Proc. Roy. Soc., 30, p. 494 and 490, 1880.

[7] Proc. Roy. Soc., 33, p. 3, 1881.

[8] Ueber die Spectren der Elemente, Zweiter Abschnitt. Also Wied. Ann., 38, p. 80, 1889.

Crew and Basquin[1] obtained strong carbon and weak cyanogen bands with an enclosed arc between metallic electrodes in CO_2. When a small quantity of air was introduced along with the CO_2, the cyanogen bands were strengthened and the carbon bands weakened. Magnesium electrodes appeared to be more favorable to the production of the carbon spectrum than did electrodes of iron and some other metals. Liveing and Dewar[2] had previously obtained similar results with an arc between carbon poles in a magnesia crucible.

The following experiments were made by the writer with an enclosed arc lamp manufactured by the Helios Electric Company. Fig. 9 shows a sectional view of the lamp as modified for the purpose of this investigation. A glass globe (*g*) was covered by a metal cap (*c*) through which passed the upper carbon. The lower end of the globe was clamped between two asbestus-lined metal plates of appropriate shape. The lower carbon extended through the plates and the frame of the lamp. A solid upper carbon was used. The lower carbon, which was hollow, had several small holes (*h*) bored through its walls near the upper end, so that a gas blown in at the lower end would be introduced into both the arc and the globe. The lamp was not air-tight, but the circulation was very slow when the lower end of the hollow carbon was closed. When a gas was blown into the globe, the admission of air was prevented by the escaping gas.

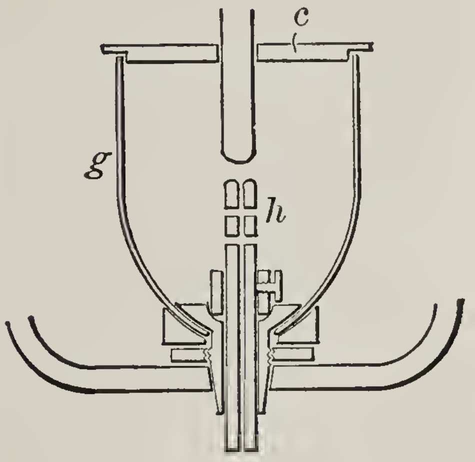

Fig. 9.

When experimenting with poisonous or offensive gases a fan motor (*F*, Fig. 3) was placed in the window of the closet to maintain a strong outward air current.

When the spectrum was photographed immediately after starting the arc, it was identical with the spectrum of the open arc. After a minute or two the arc shortened and the metallic lines began to disappear. After ten minutes a hundred volts would maintain an arc scarcely 1 cm. long. *The metallic lines had disappeared almost completely.* The carbon and cyanogen bands were even stronger than in the first photograph. Air was then passed through the hollow carbon into the arc and globe. The arc lengthened and the metallic lines reappeared. When the current of air was shut off and a stream of CO_2 turned on, the metallic lines were weakened, but they did not disappear as long as the CO_2 was flowing.

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[1] Astrophysical Journal, 2, p. 103, 1895.

[2] Proc. Roy. Soc., 34, p. 123, 1882.

It appears that a comparatively rapid disintegration of the carbon poles is necessary to furnish enough material in the arc to bring out clearly the metallic lines, which are due to very small quantities of the metals contained in the carbons as impurities. The rapidity of the disintegration depends upon the amount of O present in the globe. After the O had become exhausted by allowing the arc to burn a few minutes the wasting away of the poles was very slow, indeed. It was not more than 0.1 cm. per hour for the positive carbon, and very much less for the negative. When CO_2 was introduced the wasting of the poles increased, and, as noted, before the metallic lines appeared. Air still further increased the disintegration of the poles and the brilliancy of the metallic spectra. When pure O was passed into the globe and arc the poles were rapidly consumed. The metallic spectrum was very bright, likewise the carbon and cyanogen bands. When the lower hollow carbon was replaced by a carbon with a sulphur core (made by pouring hot sulphur into a hollow carbon) S vapor filled the globe and displaced the air very soon after the arc was started. The metallic lines did not appear, but the carbon and cyanogen bands were strong.

It was thought that the cyanogen bands might have been due to the nitrogen of the air contained in the porous carbon poles. To remove the air the carbons were placed in a small air-tight iron cylinder which was connected by tubes with an air pump and a cylinder of CO_2. The cylinder containing the carbons was placed in a furnace and kept red-hot for two hours; the air pump was worked continuously. The carbons were then allowed to cool in an atmosphere of CO_2, in which they remained for several days. The operation was then repeated and the carbons cooled as before.

When the treated carbons were placed in the lamp and a continuous stream of CO_2 was passed into the arc and globe, the cyanogen bands appeared to be a trifle weaker and the carbon bands a little stronger than with untreated carbon poles. However the difference was very slight. A like result was obtained when, instead of CO_2, O and the vapor of S were passed into the arc. A continuous stream of S vapor was obtained by attaching to the end of the hollow carbon a glass tube sealed at the lower end and filled with S. A Bunsen burner kept the S boiling vigorously, the vapor passing into the globe and arc through the hollow carbon which was heated by a second burner to prevent condensation of the vapor before reaching the arc.

It appears that the weight of evidence favors the view that the three bands in question are intimately connected with the presence of N in the arc. But there are reasons for hesitating to accept the conclusion that they are due to cyanogen or to any other compound of C and N. One reason is that the bands are found in the spectrum of the sun. It is difficult to believe that a carbon nitrogen compound exists at so high a temperature, or even at the temperature of the arc. Another reason is that the strength of the bands does not vary much when the quantity of N in the arc is varied.

All the investigations on this subject show that a mere trace of N is sufficient to bring out the cyanogen bands quite clearly while the carbon bands are weak. Flooding the arc with N does not greatly increase the strength of the cyanogen bands or weaken the carbon bands. The following experiment emphasizes this point:

Mercuric cyanide (30 gms.) was placed in a three bulb combustion tube. One end of the tube was sealed and the other connected by a rubber tube to the hollow carbon of the enclosed arc. The tube was heated by gas burners, and as soon as the cyanogen had displaced all the air in the globe the arc was started. The three cyanogen bands were very slightly, if any, stronger than in the arc in air, and not very much stronger than when treated carbons were used with CO_2 streaming into the arc and globe. Yet the quantity of cyanogen in the arc must have been hundreds of times greater in the first case. That the two spectra should be at all comparable in intensity is not in agreement with our knowledge of spectra in general.

The author inclines to the view that *the so-called cyanogen bands are due to carbon in the presence of nitrogen, but not combined with it.* It is well known that the spectrum of an element is sometimes greatly changed in the presence of another element, though there be no tendency for the elements to combine. The statement need not be confined to spectra. A very familiar instance is the addition of MnO_2 to $KClO_3$ in the generation of O. The temperature at which the O is given off is greatly reduced although the MnO_2 does not decompose nor does it combine with the K.

It has been shown that cyanogen is produced by an arc in air or N. However, the compound may be formed in the outer cooler portions of the arc and not in the central hot region where the cyanogen bands are strongest. The fading out of these bands in the outer regions is more rapid than would be expected if cyanogen were a stable compound in the arc. And to account for their production by a mere trace of N there must be a decided tendency for the compound to form. But experiment shows that cyanogen may be decomposed in the arc.

Copper rods were substituted for the carbons of the enclosed lamp. The lower rod was hollow. Cyanogen was passed through it into the arc and globe. The cyanogen bands were strong. *The carbon bands at* $\lambda=4737$ *and* $\lambda=4382$ *appeared as bright as in the enclosed arc between carbon terminals.*

It may be urged that the decomposition of the cyanogen was brought about by the presence of Cu, and not by heat alone. If this be true it would seem that the metallic impurities ordinarily present in carbon would prevent the formation of cyanogen in an arc where N was present in traces only.

Many substances besides N appear to affect the spectrum of carbon. Sulphur is an instance. When sulphur vapor was forced into the arc it seemed to tend to equalize the band spectrum, somewhat diminishing the intensity of the lines on the side of the bands next the heads, and strengthening the weaker lines on the more refrangible side. The bands were widened until the "grating effect" became continuous in the region of the spectrum photographed. S vapor alone, when forced into an arc between copper poles, gave a faint "grating effect" in the same region.

The effect of one substance upon the spectrum of another has not yet been studied systematically. Liveing and Dewar[1] found that the spectra of mixed vapors differed from the spectra of the substances themselves. Hartley[2] studied the spectra of moistened graphite poles surrounded by various gases, and when immersed in different solutions. He found that the carbon spectrum was modified most by saline solutions, and seemingly in proportion to their strength. Miss Baldwin[3] found that the band spectrum of the arc was weakened by the more positive metals, such as K, Na, Li, Ba, Ca and Sr. Bohn[4] asserts that "it is not possible to ascribe to carbon compounds *any* definite band spectrum." In reviewing Hartley's paper referred to above, Ames[5] remarks that "there is at the present time no more fruitful field open to research than that of the study of the influence of the presence of one substance upon the spectrum of another."

This investigation was made under the direction of Professor Edward L. Nichols, to whom I would express my thanks for the aid extended me and for the facilities placed at my disposal.

Physical Laboratory, Cornell University, May, 1897.

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[1] Proc. Roy. Soc., 34, p. 428.

[2] Phil. Trans., Part I., 1884, p. 49. Proc. Roy. Soc. 55, p. 344, 1894.

[3] Physical Review, III., p. 448, 1896.

[4] Ztschr. f. physik. Chem., 18, p. 219, 1895.

[5] Astrophysical Journal, I., p. 88, 1895.

CERTAIN PROPERTIES OF HEAT WAVES OF GREAT WAVE-LENGTH. II.

(*Concluded.*)

By H. Rubens and E. F. Nichols.

Rocksalt.

THE experiments with rocksalt were by far the most difficult undertaken and have proven, so far, only partially successful. The difficulty in obtaining adequately plane and highly polished surfaces, and their preservation in use, from atmospheric deterioration, was very great. In this we, at last, succeeded by employing a number of unusually pure crystals of salt from Strassfurt, and finally obtained five or six faultless surfaces which withstood, unprotected, during several weeks any considerable deterioration.

Rays from the zirconium lamp after reflection on three such surfaces retained only about one ten-thousandth of their original energy and were still able to traverse thin rocksalt plates with no greater apparent hindrance than ordinary heat rays suffer. Of rays after four reflections, 60 per cent. could still traverse considerable thicknesses of rocksalt. After five reflections rays were finally obtained, only 10 per cent. of which passed unabsorbed through thin rocksalt plates. The five reflections so reduced the energy present that the bolometer gave a throw of only 5 mm. for the total energy. From this result, the necessity of eliminating all diffused and accidental radiation from the bolometer strips, was apparent. To accomplish this, the following arrangement of apparatus was chosen: The rays from the zirconium plate *a* (Fig. 7), were rendered slightly convergent by a concave minor *b* and then reflected from a rocksalt surface at p_1, which reduced the energy to one-twentieth of its original value, after which the rays entered a black wooden box containing the remaining four rocksalt surfaces (p_2—p_5) and the bolometer. Two wooden partitions were so arranged within the box, as to effectually cut off any rays from reaching the exposed bolometer which

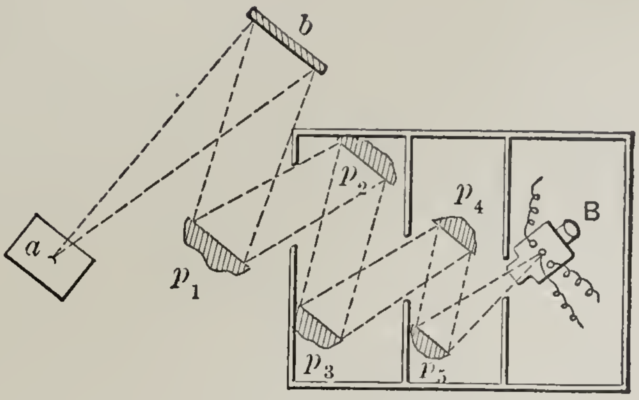

Fig. 7.

had not come from the lamp, and suffered five successive reflections on rocksalt surfaces. Under these conditions, the bolometer did good service and held very steadily to a fixed zero point, so that out of a series of ten observations the mean could be regarded as accurate to 0.2 mm.

With this small quantity of energy it seemed a hopeless task to search through the diffraction spectrum for the maximum necessary to a direct measurement of the wave length. Other experiments described in Part III made it possible, in another way, to reach an estimate of the wave length involved.

Other Substances.

We were not successful in discovering bands of metallic reflection in any of the remaining seven substances, but the search was conducted with the radiometer with a chloride of silver window, so that the finding of any bands lying farther out than the end of the transmission spectrum of silver chloride would have been impossible. From these observations we are therefore only justified in concluding that these substances possessed no regions of metallic reflection for wave lengths less than 25 μ, but it is quite possible that further search by the bolometric method just described might show regions of metallic reflection lying still farther out.

III.

Of the heat rays of various wave lengths already discussed, we have selected those resulting from four reflections on fluorite with which to make a study of the absorption, reflection and refraction of a number of different media. In contrast with the rocksalt rays, the rays from fluorite can be obtained in sufficient intensity to measure these coefficients with an accuracy approaching that attained in similar measurements in better known parts of the spectrum.

Absorption.

A study of the transmission of the fluorite rays in a number of substances has been made, and a part of the results are given in Table VII which requires no explanation.

Table VII.

Substance.	Thickness in mm.	Transmission in percentage of incident rays.	Substance.	Thickness in mm.	Transmission in percentage of incident rays.
Fluorite . . .	4.4	0.0%	Quartz . . .	2.25	0.0%
Rock salt . .	1.92	11.0	Mica	0.015	9.9
" . .	5.85	2.1	"	0.032	1.3
" . .	14.0	0.0	Glass	0.10	0.4
Sylvine . . .	3.6	34.0	Paraffin . . .	4.4	0.0
" . . .	6.3	17.8	Ebonite . .	2.0	0.0
" . . .	14.3	4.6	Sulphur . . .	2.0	0.0
Silver Chloride	0.25	77.4	Gold leaf . .	approx.	
" "	0.45	52.8		0.0001	0.0
" "	1.7	43.7	Aluminum-	approx.	
Quartz	0.018	15.7	leaf .	0.001	0.0

With the exception of silver chloride, sylvine, and rocksalt none of the substances examined showed measurable transmission in layers approaching 1 mm. in thickness. In very thin layers, quartz, mica, and glass show slight transmission. The plates of fluorite, paraffin, ebonite and sulphur, as well as the two very thin sheets of gold leaf, and aluminum were totally opaque.

In the transmission of layers of varying thicknesses of the same substance, agreement with the law of absorption is very incomplete. This is doubtless because the rays in the first place are not strictly homogeneous, and in the case of silver chloride, none of the plates studied were entirely free from air bubbles and enclosed particles of foreign substances so that here the discrepancy between the law of absorption and the observed transmission is especially marked. The behavior of lamp black, carbon dioxide, and water vapor toward these rays has been earlier noted.[1]

Experiments with rays after five reflections on rocksalt surfaces, show that nearly all substances absorb these rays in even greater measure than those from fluorite. Rocksalt, sylvine, silver chloride, and fluorite in very thin layers absorbed these rays totally, also sulphur, ebonite, glass and quartz, when one or more millimeters in thickness. Of all the substances examined, paraffin was most diathermous, transmitting 50 per cent. through plates 1.5 mm. thick. We hope later by the aid of a radiometer with a thin paraffin window to measure the wave length of these rays.

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[1] Rubens and Nichols, l. c.

Reflection.

For the reflection measurements, the arrangement of apparatus shown in Fig. 8 was used, and the measurement was a comparison of the reflection of a plane surface of the substance studied, with that of a polished silver plane mirror.

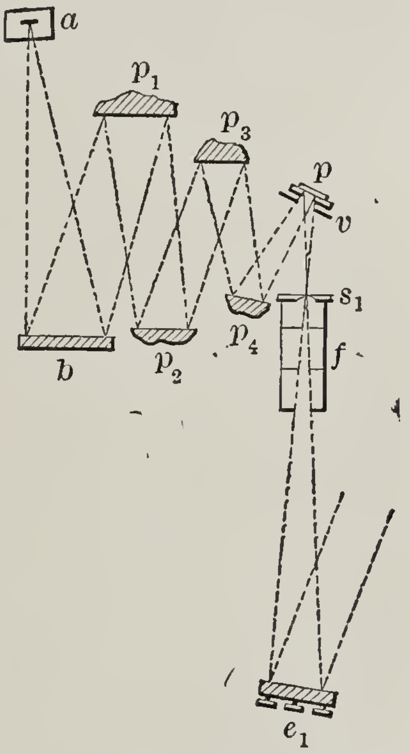

Fig. .

The rays from the burner at *a*, after reflection on the fluorite surfaces p_1—p_4, were reflected from the surface *r* of the body in question into the slit s_1 of the spectrometer. The accurate replacing of the surface *r* with that of the comparison mirror, was accomplished by means of a plane brass plate with two equal apertures, one above the other. This plate slid on a carefully constructed bearing, so that the plane remained unchanged. Two stops, placed on the bearing, allowed the plate to slide over a distance equal to that between the centers of the two circular apertures, so that when the plate struck against the upper stop, the lower aperture was in the same position as the upper aperture when the plate was in contact with the lower stop. Surfaces to be compared were brought in contact with the back surfaces of the plane brass plate behind the apertures. Thus the reflection of each substance studied was compared with silver, and, from time to time, two equal silver mirrors were compared to make sure that the adjustment favored neither of the apertures above the other. To further exclude any inaccuracy which might arise from a slight displacement of the stops, a permanent diaphragm *v* with an opening smaller than the apertures in the plate, was mounted directly in the path of the rays in front of the movable plate.

The reflection of silver was regarded throughout these measurements as total, an assumption in the justification of which the following considerations may be offered: First, the reflection of silver in the infra red as well as in the visible spectrum has been shown to increase with the wave length. At 3 μ its reflection already exceeds 98 per cent. and at 4 μ is more than 99.[1] Second, the reflection of silver has been carefully compared with that of a number of other metals for rays of 23.7 μ wave length and it has been found that within the limits of observational error the reflection coefficient of every metal studied was equal to that of silver itself. Table I shows the results of these measurements, to which the reflection percentages of a number of the same metals for green light has been added to afford an interesting comparison.[2]

TABLE VIII.

Metal.	Reflection percentage. $\lambda = 23.7\mu$	Reflection percentage. $\lambda = 0.50\mu$
Silver	100.0	88.3
Gold	99.8	56.1
Platinum	100.0	—
Copper	100.7	54.8
Iron	99.5	57.7
Nickel	100.3	61.0
Brass	100.0	—
Speculum metal	100.1	—

From the table it appears probable that, not only the reflection of silver but also that of the remaining metals and alloys have, at this wavelength, practically reached 100 per cent—the theoretical limit.

In the same way the reflection of seven of the substances contained in Table VII was measured. All of these substances, except fluorite, showed, by a slight transmission, that the relation between reflection and refraction in the Fresnel formula held without sensible error. As the results could, at best, be only close approximations, the simplified form of the equation—for normal incidence—was used in the computation of n, although the reflection measured took place under an angle of about 13°.

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[1] H. Rubens, Wied. Ann., 37 S., 249, 1889, and E. F. Nichols, PHYSICAL REVIEW, 297, IV., p. 22, 1897.

[2] H. Rubens, Wied. Ann., 37 S., 265, 1889.

The results appear in Table IX.

Table IX.

Substance.	Percentage of reflection.	Index of Refraction n computed.
Fluorite	71.5	—
Quartz	41.7	4.62
Mica	28.7	3.32
Glass	19.7	2.55
Sulphur	11.4	1.95
Sylvine	2.0	1.32
Rock-salt	1.7	1.22

According to the observations, fluorite shows a mean reflection of 71.5 per cent. For rays in the middle of the absorption band the reflection is doubtless much greater, probably rivalling that of silver in the visible spectrum. The high reflection of quartz and mica, and the computed values of the refractive index for these materials, are doubtless due to bands of metallic absorption, which, in both substances, occur in the immediate neighborhood—in quartz at 20.7 μ and in mica at 21.25 μ. In both, the bands lie on the side of the shorter wave lengths. The indices of refraction for glass and sulphur lie very near the square roots of their dielectric constants, the agreement in the former case, however, may well be accidental.

As might well have been expected from earlier computations of their dispersion constants, surfaces of rocksalt and sylvine, by their reflection, show a much lower index of refraction than the square root of the dielectric constants would give. The reason for the discrepancy will be shown later.

Refraction.

The transmission of only three of the substances so far examined—sylvine, rocksalt, and chloride of silver—was sufficient to make the direct determination of refraction index, by means of a prism, possible. Of these three, silver chloride was excluded on other grounds.

In sylvine and rocksalt the absorption was so great that it was necessary to use prisms of very acute refracting angles—for rocksalt $\varphi = 10° \ 53'$ for sylvine $\varphi = 12° \ 39' \ 10''$. The surfaces of the prisms obtained were plain, and sufficiently polished to permit the determination of the prism angle, accurate to within 10″, by the Gauss eyepiece. The prism angle and the refractive index for sodium light were determined, in both prisms by the aid of a spectrometer of the usual type. The refractive indices observed agree with the similar measurements, by one of the present writers,[1] to within one unit in the fourth decimal place for rocksalt, and to within two units for sylvine. The agreement is very satisfactory, when the small refracting angle of the prisms is taken into account. The prisms were next placed on the table of the reflecting spectrometer $s_1 \ e_1 \ e_2 \ s_2$, in place of the grating at g (Fig. 1), and by the radiometer, the deviation of the energy maxima for quartz and fluorite rays was observed, first on the right, the prisms reversed, and the same operation repeated on the left. In the quartz spectrum the deviation of the maximum at 20.75μ, and of the maximum in the fluorite spectrum are given for rocksalt in Table Xa, and for sylvine in Table Xb.

TABLE Xa.

ROCK-SALT.

Refracting angle = 10° 53′ 0″

Series.	Band I (Quartz)	Band II (Fluorite)
I	$R = 2° \ 27\frac{1}{2}'$ $L = 2 \ \ 28$	$R = 2° \ 14\frac{1}{2}'$ $L = 2 \ \ 15\frac{1}{2}$
II	$R = 2 \ \ 28$ $L = 2 \ \ 29$	$R = 2 \ \ 15$ $L = 2 \ \ 15$
Mean	2° 28′ 8″	2° 15′ 0″

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[1] H. Rubens, Wied. Ann. 45, p. 238, 1892, and H. Rubens and B. W. Snow, Wied Ann. 46, p. 529, 1892.

TABLE Xb.

SYLVINE.

Refracting angle = 12° 39′ 10″

Series.	Band I (Quartz)	Band II (Fluorite)
I	$R = 2°\ 59'$ $L = 3\ \ 0$	$R = 2°\ 51'$ $L = 2\ \ 51$
II	$R = 2\ \ 59$ $L = 2\ \ 59\frac{1}{2}$	$R = 2\ \ 50$ $L = 2\ \ 50\frac{1}{2}$
Mean	2° 59′ 22″	2° 50′ 37″

The letters *R* and *L* in the tables denote deviations to the right and to the left sides respectively. The deviation of the sodium line picked up by the radiometer was 3° 35′ 58″ for the rocksalt prism and 3° 46′ 40″ for the sylvine prism. These deviations were all measured with the spectrometer arranged with the fixed slit s_2 (Fig. 1), and as has already been described in Part I, must be multiplied by a constant factor to gain the corresponding true deviations δ. This factor was redetermined by the aid of the grating, after the present observations were made and was found to be 1.6540.[1] The values of δ obtained by multiplication from values of α, together with the refractive indices corresponding to the computed deviations, are given in Tables XIa and b.

TABLE XIa.

ROCK-SALT.

	Observed deviation α	Corrected deviation δ	n
Sodium	3° 35′ 58″	5° 57′ 15″	{ 1.5437 (1.5439)
Band I. (Quartz) . .	2 28 8	4 5 0	1.3735
Band II. (Fluorite) .	2 15 0	3 43 18	1.3403

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[1] The difference between this value and the value 1.6522 on page — is due to a slight change in the position of the slit s_2.

TABLE XIb.

SYLVINE.

	Observed deviation α	Corrected deviation δ	n
Sodium	3° 46′ 40″	6° 15′ 0″	1.4902 (1.4905)
Band I. (Quartz) . .	2 59 23	4 56 42	1.3882
Band II. (Fluorite) .	2 50 37	4 42 12	1.3692

The values of n for the sodium line which appear in parenthesis were the values observed with the ordinary spectrometer mentioned earlier. This agreement of the values obtained by the two methods serves as a check upon the accuracy of the measurements here given.

A more exact determination of the wave lengths of the maxima of the two bands I. and II. in the prismatic spectrum requires not only a knowledge of the dispersion in those regions, but also a knowledge of the selective absorption in both cases. It is, however, only necessary to know these approximately, in order to gain a very close estimate of the wave lengths involved, especially as concerns band I., where the correction to the wave length, as measured in the grating spectrum, will be less than 1 per cent. The displacement of grating spectrum maximum of band I. due to the absorption of the prisms as measured by one of the present writers, was for rocksalt 0.10μ and for sylvine only 0.05μ—a wave length correction of 0.5 and 0.3 per cent. respectively. The effect of selective absorption to displace the maximum of band II., was much greater in both cases, for rocksalt 1.1μ and for sylvine 0.9μ.

The correction for varying dispersion in these regions is small—for band I. 0.4 per cent., for band II. 1.2 per cent. The corrections were made by plotting the dispersion curves for both prisms. This was done by using the wave lengths of both bands, corrected for absorption, and the corresponding indices of refraction, and also, by the use of the earlier measurements of dispersion by one of the present writers. The dispersion curve gives values sufficiently exact of the differential coefficient $dn/d\lambda$ by which the ordinates of the energy curve of the grating spectrum were divided, and the distribution of energy in the prismatic spectrum obtained. This presupposes that $dn/d\lambda$ is proportional to $d\delta/d\lambda$ which, taken strictly, is true, only for a prism of infinitesimal refracting angle, in which case

$$n=\frac{\sin\frac{\varphi+\delta}{2}}{\sin\frac{\varphi}{2}}=\frac{\varphi+\delta}{\varphi}$$

$$\text{and}\quad \frac{d\delta}{d\lambda}=\frac{d\delta}{dn}\,\frac{dn}{d\lambda}=\varphi\frac{dn}{d\lambda}$$

These equations still hold with a sufficient approximation for $\varphi=12°$, to justify their use in the present case. When all the corrections are taken into account, the wave lengths of the maxima of bands I. and II. in the rocksalt prism were at $\lambda=20.57\mu$ and 22.3μ, and for the sylvine prism at $\lambda=20.60\mu$ and 22.5μ respectively. The accuracy of these values is very unequal. The probable error for band I. is, in both prisms, about 0.3 per cent., while for band II. the results can scarcely be regarded as certain within 1.0 per cent.

In the computation of the constants in the Ketteler-Helmholtz dispersion formula (1) which we sought to determine in connection with these values, only the values for band I. were used in addition to the earlier dispersion measurements by one of the present writers. The constants M_1 and λ_1 which have very little influence upon the course of the curve beyond $7.\mu$, were taken from the earlier measurements. The values of M_2 and λ_2, on the other hand, were newly determined from two values of the refractive index in the infra-red spectrum, one of which was the new value for band I. The constant b^2 was finally determined by addition from a^2 and M_2/λ_2^2. In Tables XIIa and b, the observed indices of refraction for a number of wavel engths in the visible and infra-red spectrum, are compared with corresponding values computed by the aid of equation (1). As an interesting comparison the values computed from the simplified formula:

$$n^2=a^2+\frac{M_1}{\lambda^2-\lambda_1^2}-k\,\lambda^2 \qquad (3)$$

and the constants of the earlier measurements are given also. The numbers turn out much too large for the greater wave lengths, while values from (1) agree satisfactorily with all observed values.

TABLE XIIa.

ROCK-SALT.

λ	n observed.	n comp. (1)	n comp. (3)
0.434μ	1.5607	1.5606	1.5606
0.589	1.5441	1.5441	1.5441
8.67	1.5030	1.5030	1.5030
20.57	1.3735	1.3735	1.3926
22.3	1.340	1.3403	1.3679

$b^2 = 5.1790$, $M_1 = 0.018496$ $M_2 = 8977.0$
$\lambda_1^2 = 0.01621$, $\lambda_2^2 = 3149.3$

TABLE XIIb.

SYLVINE.

λ	n observed.	n comp. (1)	n comp. (3)
0.434μ	1.5048	1.5048	1.5048
0.589	1.4900	1.4899	1.4899
7.08	1.4653	1.4653	1.4654
20.60	1.3882	1.3882	1.3948
22.5	1.369	1.3688	1.3789

$b^2 = 4.5531$, $M_1 = 0.0150$, $M_2 = 10747$
$\lambda_1^2 = 0.0234$, $\lambda_2^2 = 4517.1$

Of the constants determined for rocksalt and sylvine from equation (1) λ_2 is important, for it must represent approximately the wave length of the infra-red absorption band in these materials and consequently the wave length of rays after multiple reflection on these substances. Hence the wave length of rays, after five reflections from rocksalt, should be $\sqrt{3149.3} = 56.\mu$. From the relation between the values of λ_2 from equation (1) for quartz and fluorite, to the observed values, it is probable that $56.\mu$ is too great, and that the wave length, when measured, will be found not far from $50.\mu$.

As has been pointed out by F. Paschen, the constant b^2 should represent the dielectric constant for each substance, as it is the square of the index of refraction for an infinite wave length. We are now in position to add three more substances to the list containing the value of b^2 observed by Paschen for fluorite, and for flint glass by one of the present writers.

These values are given in Table XIII, together with values of the dielectric constant as observed by other methods. The names of the observers are added in the last column.

Table XIII.

Substance.	b^2	Dielectric constant κ	Observer.
Flint-glass	6.77	6.7 — 9.1	Hopkinson.[1]
Fluorite	6.09	6.8 6.7 6.9	Curie.[2] Romich u. Nowak.[3] Starke.[4]
Quartz	4.58	4.55 4.6 4.73	Curie. Romich u. Nowak. Starke.
Rock-salt	5.18	5.85 5.81 6.29	Curie. Thwing.[5] Starke.
Sylvine	4.55	4.94	Starke.

The closest agreement between b^2 and the dielectric constant occurs in quartz in which the data for the computation of the dispersion constants are fullest. When the uncertainty connected with the computation of b^2 and also in the methods for finding the dielectric constant by direct observation taken into account the agreement in the remaining instances is as good as could have been expected.

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[1] Hopkinson, Phil. Trans. 2, p. 355, 1881.

[2] Curie, Ann. Chim. Phys. (6) 17, p. 385, und 18, p. 203, 1889.

[3] Romich u. Nowak, Wien. Ber. (2) 70, p. 380, 1874.

[4] H. Starke, Dissertation, Berlin, 1896.

[5] H. Rubens, Wied. Ann. 45, p. 238, 1892.

IV.

Electro-magnetic Experiments.

Encouraged by the relatively great length of the rays from fluorite the success of an investigation to discover electrical resonance seemed by no means impossible. The experiments made were closely analogous to those performed by A. Garbasso[1] in 1893, in which the reflection of electrical waves from wooden walls covered with a number of uniformly distributed parallel electrical resonators was studied. Garbasso found that such walls showed a high reflection percentage only when the period of the vibrator was equal to that of the resonators. The study was made with electrical waves 43 and 70 centimeters long.

The production of plates of resonators bearing the same relation to the lengths of our waves which, although relatively very long, were in reality very short was attended by the greatest difficulties which only after many unsuccessful attempts we were able in a measure to overcome.

A number of glass plates with plane surfaces were silvered by the usual chemical process. Each immediately after silvering was ruled with a diamond point on the dividing engine into a grating with 100 lines to the milimeter. The width of the line cut through the silver coating was made as nearly as possible equal to the width of the remaining strip of silver so that both were approximately 5μ wide. It was usually possible to attain this end with sufficient exactness by carefully choosing the diamond point and by regulating the pressure for each individual silver coating supposed in itself to be uniformly coherent.

Gratings obtained in this way were turned through a right angle and ruled with lines perpendicular to the former ones. The grating was thus cut into a great number of minute rectangular fields of silver to serve as resonators. No cost in patience or time was spared to make these fields for each plate as nearly uniform as possible. Five such plates were finally obtained with resonators of as many different lengths. The length and width of the individual resonators and the number to each square centimeter of surface are given in Table XIV.

[1] A. Garbasso, Accad. delle Scienze di Torino, 1893. Vol. XXVII.

Of the five plates of resonators which finally came to measurement, one was a grating, *i. e.*, contained resonators which compared with the wave length were practically of infinite length. The remaining four plates were covered with resonators the length of which varied from plate to plate by about 6μ. The surface on each plate covered with resonators was $(1.5\ \text{cm.})^2$ or $2.25\ (\text{cm.})^2$.

The study of a greater number of plates of resonators which would have been in many respects desirable was hindered by the great difficulties encountered in ruling the plates, chief of which was the difficulty found in getting silver coatings of uniform coherence on different portions of the same plate. By far the greater part of the resonator plates made according to the method described were found on examination to be useless, either the diamond point had not cut through the silver coating at all points, or the metal in many places had been torn entirely away. Both faults often occurred on the same plate. The resonators on the plates finally studied were by no means everywhere intact yet with the exception of Plate V., the proportion of injured resonators was not large enough to materially affect the results. Of the resonators on Plate V. nearly 10 per cent were imperfect so that the reflection obtained for this plate was unquestionably too small. The cross rulings in this grating were far enough apart ($\lambda + 5\mu$,—the length of the resonator plus the width of the line) so that diffraction images of the first order were possible; a source of error from which the other plates were entirely free. The effect of this error upon the measured reflection of Plate V. would also be to make it too low by the amount lost through diffraction.

The same arrangement of apparatus shown in Fig. 8 was used here except that a plane glass plate was so introduced into the path of the rays between p_4 and r that the middle rays in the bundle were reflected at the angle of complete polarization[1] before reaching the resonators at r.

In every case the reflection of the resonator plates was studied in two positions at right angles to each other. First with the electrical component of the polarized rays parallel to the long way of the resonators, and second, with the electrical component and long way of the resonators at right angles.

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[1] The index of refraction n of the glass plate d was computed from its reflection percentage ρ by means of the Fresnel formula. In this case $\rho = 20.2$ per cent. and $n = 2.62$. The transmission of very thin plates of glass for these rays was sufficient to warrant neglecting the absorption in the computation of n.

Table XIV gives the results obtained and also the dimensions and characteristics of the resonators of the different groups.

Table XIV.

Number of Resonator Plate.	I	II	III	IV	V
Number of Resonators per sq. centimeter	1×10^3	1000×10^3	572×10^3	400×10^3	333×10^3
Length (l) of Resonators . .	∞	6.5μ	12.4μ	18.0μ	24.4μ
Width (w) of Resonators . .	5.8μ	4.6μ	5.3μ	5.1μ	5.5μ
Reflection percentage electrical component perpendicular to long way	40.8	38.3	42.7	40.7	36.1
Reflection percentage electrical component parallel to long way of Resonators .	83.7	41.8	65.8	49.5	62.5

The values obtained for the reflection of the different plates may be regarded as consisting of two parts, the one due to the reflection of the resonators themselves and the other due to the reflection of the glass plate. The latter component is by no means negligible, for in the present case it may reach 24.7 per cent., which was shown by repeated measurements to be the reflection of the glass plates before silvering.

To get at the approximate reflection of the resonators independent of this background we may proceed as follows: Let α be the observed reflection percentage of resonators and glass combined. Let β be the part of this due to the resonators alone and γ the part reflected by the glass, then

$$\alpha=\beta+\gamma.$$

The part not reflected by the resonators $100-\beta$ will be the intensity

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[1] The index of refraction n of the glass plate was computed from its reflection percentage ρ by means of the Fresnel formula. In this case $\rho=20.2$ per cent. and $n=2.62$. The transmission of very thin plates of glass for these rays was sufficient to warrant neglecting the absorption in the computation of n.

of the rays incident on the glass, and of this amount 24.7 per cent. will be reflected; hence

$$\gamma=(100-\beta)\,0.247$$

and

$$\beta=\frac{\alpha-24.7}{0.753}$$

The formula rests upon two assumptions which have never been proven: First, that the resonators do not absorb and, second, that the glass surface and resonators reflect independently, neither disturbed by nor disturbing the other. The formula is in consequence unassailable only in the limiting cases where $\beta=0$ or 100, and between these limits it may be regarded as an interpolation formula, giving only a first approximation to true values.

From the values of α in Table XIV values of β have been computed by the formula and are given in Table XV.

Table XV.

Number of resonator plate.	I	II	III	IV	V
β, Electrical component perpendicular to long way of resonators	21.4	18.1	23.9	21.3	15.3
β, Electrical component parallel to long way of resonators	78.4	22.7	54.5	32.9	50.2

A glance at the table shows that the values in the line representing the reflection of the different resonator plates varied between comparatively narrow limits when the electrical component was perpendicular to the long way of the resonator. If for reasons already given the value for Plate V be excluded, the limits of variation become 18.1 and 23.9 per cent. On the other hand, the numbers in the line showing the reflection when the electric component was parallel to the long way of the resonators show great variation, not only when compared with corresponding values in the line above, but also among themselves. The result of the comparison leads to the following conclusions:

1st. All the resonators show a higher reflection when the electric vector and the long way of the resonators coincide, than when at right angles to each other.

2d. The resonators of Plates I, III and V show a much stronger reflection than Plates II and IV when the electric vector lies in the long way of the resonators ; when at right angles the difference is small.

3d. The highest reflecting power is exhibited by the resonators of Plate I which, in comparison with the incident wave length, may be regarded as infinitely long.[1]

The results may easily be brought into satisfactory agreement with the requirements of the electro-magnetic theory of light. We can think of the long resonators of Plate I. as divisible with sufficient accuracy by a very great whole number of half wave lengths for all the observed wave frequencies between $\lambda=22$ and 28μ and thus these resonators are theoretically to be regarded as representing the most favorable conditions for perfect resonance.

The remaining results are further in agreement with results reached by earlier experiments[2] with electrical waves in which a higher degree of resonance was observed when the resonators approached a whole number of half wavel engths than when near an odd number of quarter wave lengths. The length of the resonators of Plates II, III, IV and V represented approximately 1, 2, 3 and 4 quarter wave lengths respectively.

That the observed resonance phenomena were not even more sharply defined and definite than they appear here can doubtless be accounted for by a more careful consideration of the conditions under which the experiments were made. In the first place we have no assurance that the period of the resonators of plates III and IV corresponded to that of the energy maximum of the incident rays. The mathematical computation of the length of maximum resonance for resonators of the form used has never been made and it is questionable if in such a computation one may neglect the influence of each resonator upon the period of its neighbors and vice versâ. Second, the relation of length to width in the resonators used would probably lead to a heavily damped vibration due to a relatively large capacity and low self-induction. Third, the incident beam was a convergent bundle and therefore only the middle part could have been completely polarized by reflection. Lastly, the incident rays were not strictly homogeneous.

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[1] That gratings of parallel wires exert a similar polarizing action upon transmitted infra-red rays was shown in 1892 by Dr. Du Bois and one of the present writers—H. Rubens, (Wied. Ann., 49, S593, 1893). The polarizing action then observed, because of the shorter waves employed, was very much smaller.

[2] A. Righi, Rendic. R. Acc dei Lincei (6) 2, p. 505, 1893.

Under more favorable circumstances we think a higher degree of electrical resonance might confidently be expected, although we believe that electrical resonance as a property of heat rays has been proved beyond question by the experiments here described.

The writers, in closing, wish to express their appreciation of the kindness of President F. Kohlrausch and of Professors E. Warberg and A. König who lent several pieces of valuable apparatus used in carrying out the present study.

We wish further to thank Mr. Augustus Trowbridge for kindly assistance in the ruling of a part of the resonator plates.

Physical Laboratory of the Charlottenburg Polytechnic Institute
January, 1897.

THE SURFACE TENSION OF WATER AND OF CERTAIN DILUTE AQUEOUS SOLUTIONS, DETERMINED BY THE METHOD OF RIPPLES. I.

By N. Ernest Dorsey.

Introduction.

PREVIOUS observers have found that the surface tension of most aqueous solutions is a linear function of the concentration; but they have worked on no solution more dilute than about one-half normal (a normal solution is one that contains in one liter of the solution as many grams of the salt as there are units in the molecular weight of the salt, the molecular weight of hydrogen being taken as two). It seems, therefore, desirable to determine, if possible, the surface tension of more dilute solutions; and this work was undertaken at the suggestion of Professor J. S. Ames in the spring of 1895, for the purpose of studying solutions as dilute as possible. Most of the time since then has been spent in studying the various methods, and in perfecting the method of ripples, which appeared to offer fewer theoretical objections than any other.

The arrangement of apparatus finally adopted gives an average departure from the mean of several observations of not more than one-seventh of one per cent. Though different samples of water may give results that differ among themselves by more than this amount, still in the entire series of twenty-one samples of water the average departure of a single observation from the mean was but one-fifth of one per cent. This compares very favorably with the results obtained by any other method.

This spring determinations of the surface tensions of a few solutions were made at concentrations varying from one-tenth normal to normal, and the values found lie upon straight lines starting from the value for pure water.

Historical and Critical.

Among those who have worked on the surface tension of aqueous salt solutions may be mentioned Frankenheim,[1] Buliginsky,[2] Valson,[3] Quincke,[4] Lippmann,[5] P. Volkmann,[6] O. Rother,[7] Traube,[8] Röntgen and Schneider,[9] Klupathy,[10] Goldstein,[11] Jäger,[12] N. Kasanskine,[13] Canestrini,[14] Sentis.[15]

Excepting Sentis, Jäger, and Klupathy, they all used the method of capillary tubes, and Quincke supplemented this with his method of large bubbles. All determinations of the surface tension from the measured rise of the liquid in capillary tubes involve an assumption in regard to the contact angle, *i. e.*, in regard to the angle included between the wall of the tube and the tangent to the liquid surface at the *point of contact with the tube.* This angle cannot be measured, for by any possible method we must measure the inclination of a finite arc, and as the surface extends on one side only of the point at which we desire the inclination, the finite arc whose inclination we measure must lie entirely on one side of the point of contact. Hence every measured value of this angle will necessarily be too large. This is probably the reason Professor Quincke obtains finite contact angles for liquids that wet the glass in contact with them.

Another objection to measuring the surface tensions of solutions at the line of intersection of three bodies, solution, air and glass, is that probably the tensions of both the solution-air and the solution-glass surfaces change with the concentration of the solution, so that the results obtained involve two changes which can not be separated. This renders the interpretation of the results very difficult. Jäger used a method depending on the relation between the diameter of a capillary tube and the distance it must be immersed in the liquid in order that the same air pressure might cause steady streams of bubbles of the same size to issue from it and from another tube of known diameter immersed to a fixed depth. He determined this relation empirically from experiments on water, and he accepted the results of Brunner and of Wolf as correct. Quincke's work on flat bubbles has been discussed in several papers and seems to be not very satisfactory. From this we see that, excepting the work done by Sentis and by Klupathy, the determinations of the surface-tensions of solutions are not all that can be desired.

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[1] Pogg. Ann., 37, 409, 1836.

[2] Pogg. Ann., 134, 440, 1868; A. ch. ph. (4), 20, 361, 1870.

[3] C. rend., 74, 103, 1872.

[4] Pogg. Ann., 160, 337 and 560, 1877.

[5] Wied. Ann., 11, 316, 1880.

[6] Wied. Ann., 17, 353, 1882.

[7] Wied. Ann., 21, 576, 1884.

[8] Jour. f. pr. ch., 31, 192, 1885.

[9] Wied. Ann., 29, 165, 1886.

[10] Math. u. naturwiss Ber. aus Ungarn, 5, 101, 1887; Wied. Ann. Beibl., 12, 750, 1888.

[11] Zeitschr. f. phys. chem., 5, 233, 1890.

[12] Acad. d. Wiss. in Wein, 100_{2A}, 493, 1891.

[13] J. de la Soc phys. chem. russe, 23, 468, 1891; Jour. de Phys. (3), 1, 406, 1892.

[14] Riv. Sc. ind., p. 33, 1892; Wied. Ann. Beibl., 16, 335, 1892.

[15] Journ. de Phys. (2), 6, 571, 1887; Thesis published at Paris, February, 1897 Jour de Phys. (3), 6, 183, 1897.

Sentis employed an entirely different method, and one that appears to be very free from objections. He draws the liquid up into a capillary tube, then removes the tube from the liquid and allows the liquid to run out so as to form a drop around the end of the tube. This drop supports a certain length liquid column in the tube. He then measures the maximum diameter of the drop, focuses a microscope on the meniscus in the tube, and slowly raises below the tube a beaker of the liquid. When the liquid in the beaker reaches the drop, the column in the tube falls; the position of the beaker is now noted and it is then again slowly raised until the meniscus in the tube has come back to its previous position, and the position of the beaker is then noted. The vertical distance between these two positions of the beaker is (after certain corrections are made) the height the liquid will rise in a tube for which the contact angle is zero and whose radius is equal to that of the drop. His individual determinations may differ by one-half of a per cent., but generally they agree very well.

In the present work it was desired to use a method entirely independent of the mutual action between the liquid and a solid; and the only methods so far devised that completely satisfy this condition are Lenard's[1] method of vibrating falling drops, the method of Eötvös,[2] that employed by Sentis, and the method of ripples, which was first successfully used by Lord Rayleigh.[3] Sentis' method, as described in his first paper[1] did not seem very satisfactory; and, of the others mentioned, Lord Rayleigh's appeared to be the most promising, and is the one finally adopted.

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[1] Wied. Ann., 30, 209, 1887.

[2] Wied. Ann., 27, 448, 1886.

[3] Phil. Mag. (5), 30, 386, 1890; Theory of Sound, Vol. II., p. 344.

METHOD OF RIPPLES.

This method is based on the relation between the surface tension of a liquid and the velocity of propagation, on its surface, of a train of small waves of given period. This was deduced by Lord Kelvin[2] for the case of infinitely small waves on the surface of a perfect liquid of given depth. His formula is

$$V^2=\left(\frac{g\lambda}{2\pi}+\frac{2\pi T}{\rho\lambda}\right)\tan\text{h}\,\frac{2\pi h}{\lambda}\cdot$$

Here V is the velocity of propagation; λ is the wave-length; T is the surface tension; ρ is the density; h is the depth of the liquid; and g is the acceleration of gravity. If n is the frequency of the waves we can write this equation thus:—

$$T=\rho\left(\frac{\lambda^3 n^2}{2\pi}\cot\text{h}\,\frac{2\pi h}{\lambda}-\frac{g\lambda^2}{4\pi^2}\right)\cdot$$

Now
$$\cot\text{h}\,\frac{2\pi h}{\lambda}=\frac{1+\varepsilon^{-\frac{4\pi h}{\lambda}}}{1-\varepsilon^{-\frac{4\pi h}{\lambda}}}=1+2\varepsilon^{-\frac{4\pi h}{\lambda}}$$

approximately if h is large.

In this work h is never less than 1.1 cm., and λ is never as great as 0.5 cm. Hence

$$\frac{4\pi h}{\lambda}\not<25 \quad \therefore \cot\text{h}\,\frac{2\pi h}{\lambda}\not>1+2\varepsilon^{-25}.$$

This shows that we may consider $\cot\text{h}\,\frac{2\pi h}{\lambda}=1$, and may write the equation

$$T=\rho\left(\frac{\lambda^3 n^2}{2\pi}-\frac{g\lambda^2}{4\pi^2}\right)\cdot$$

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[1] Journ. de Phys. (2), 6, 571, 1887.

[2] Phil. Mag. (4), 42, 375, 1871.

Lamb[1] gives the more exact formula for the case of a liquid of infinite depth

$$V=\left(\frac{\rho-\rho'}{\rho+\rho'},\ \frac{g\lambda}{2\pi}+\frac{2\pi T}{(\rho+\rho')\lambda}\right)$$

or

$$T=(\rho+\rho')\ \left(\frac{\lambda^3n^2}{2\pi}-\frac{\rho-\rho'}{\rho+\rho'}\ \frac{g\lambda^2}{4\pi^2}\right)$$

where ρ' is the density of the air in contact with the surface. For the accuracy obtained in measuring λ the first and simpler formula is sufficiently exact.

Of the terms on the right of these equations, the first is much the larger for small waves. Hence the value calculated for T will be proportional to the cube of the wave-length and to the square of the frequency. Here is the first objection to this method. The value found for T will be only one-third as accurate as the measurement of the wave-length, and one-half as accurate as the determination of the frequency. Again, since the formulæ are deduced for waves of infinitely small amplitude, we must use very moderate vibrations and these are difficult to measure.

Several, among whom we need mention only Matthiessen, Riess, and Ahrendt, have attempted to test experimentally the extreme accuracy of Lord Kelvin's formula.

When a needle is placed so that its point dips slightly into a jet of water, a series of stationary waves is formed behind the needle, and their length is equal to that of waves whose velocity is equal to the velocity of the surface of the jet at that point. This is the method employed by Ahrendt.[2] He found that the value of the surface tension calculated from the observed wave-length is too great but that it decreases as the needle is removed from the orifice. This proved that the fault lay in assuming that the surface of the jet moves with the velocity calculated for the ideal case. In reality, the surface has a smaller velocity, probably owing to friction with the edge of the orifice and with the air. Hence he proved nothing, so far as the formula is concerned.

Both Reiss[3] and Matthiessen[4] failed to satisfy two of the conditions which were assumed by Lord Kelvin in obtaining his formula. One of these is that the amplitude of the waves shall be very small in comparison with the wave-length; and the other is that the surface of the liquid may be represented by an equation of the form

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[1] Hydrodynamics, p. 446.

[2] Exner's Rep. der Phys., 24, 318, 1888.

[3] Exner's Rep. d. Phys., 26, 102, 1890.

[4] Pogg. Ann., 134, 107; 141, 375; Wied. Ann., 32, 626; 38, 118.

$$y = a \sin (mx + nt)$$

i. e., the formula is deduced for the case of a single series of very small, plane waves. The waves employed by these observers were of such an amplitude that they could be easily seen with the unaided eye; this alone might very probably introduce a discrepancy between the observed and calculated values for the wave-length. This applies to all measurements made by them. Most of their observations were made on the interference waves produced on the surface of a liquid when a vibrating fork is supported vertically above it so that two needles, one attached to either prong, may dip into the liquid. This gives two series of circular waves and so the second condition mentioned above is not fulfilled. I do not know how much this would affect the result, but until it is proved to have no affect the results must be considered as nugatory. Riess[1] says "Der Schlüssel für die definitive Lösung ist in einer minutiösen Messung der Amplituden zu suchen" and he goes on to say that the only sure method is an optical or photographic one. Matthiessen found that the formula gives results in accord with his observations except in the neighborhood of the minimum value of the velocity.

C. M. Smith[2] employed this method for determining the surface tension of mercury, but obtained very erratic results. Lord Rayleigh[3] thinks he obtained waves due the suboctave of his fork, and these are almost sure to have too large an amplitude.

W. Ochse[4] attempted to determine the surface tension of solution by means of ripples. He measured the interference pattern of two series of circular waves, as Riess and Matthiessen had done, his waves had a large amplitude, he assumed that the density did not enter into the equation, and he used rather concentrated solutions. His measurements of the wave-length are quite rough, so that the results have but little value even when corrected for the density.

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[1] Exner's Rep. d. Phys., 26, 131, 1890.

[2] Proc. Roy. Soc. Edinb., 17, 115.

[3] Phil. Mag. (5), 30, 386, 1890.

[4] Exner's Rep. d. Phys., 26, 641, 1890.

From this it is seen that the formula has not been disproved, and the fact that Lord Rayleigh obtained the same value of the surface tension of water with two forks of different frequencies, and that the result obtained for water in this work agrees, within his experimental error, with Lord Rayleigh's seems to justify the application of Lord Kelvin's formula to these two cases.

All the work so far described has been with waves easily visible, and these very probably have an amplitude too large to allow the application of Lord Kelvin's formula. Lord Rayleigh was the first to use waves invisible under ordinary conditions. This method was to have the waves generated by means of a plate of glass which was attached to the lower prong of a vibrating fork, and which dipped into the liquid to be experimented upon. Then "in order to see the waves well, the light was made intermittent in a period equal to that of the waves, and Foucault's optical method, for rendering visible small departures from truth in plane or spherical reflecting surfaces, was employed." The liquid was contained in a shallow porcelain tray, and its surface was cleaned by a flexible brass hoop wider than the depth of the liquid. This was placed in the liquid so as to include the plate that generates the ripples, but otherwise in as contracted a position as possible. It was then expanded to its maximum extent. This gives a fresh surface as clean as the body of the liquid. The mean of his results for water is $T=74$ dynes per centimeter at 18° centigrade, and this is probably correct to about one per cent.

Description of Apparatus.

I shall now describe the apparatus used in this work, and which is simply a natural development of Lord Rayleigh's, although it contains many new features.

The arrangement of apparatus is represented diagrammatically in Fig. 1. S is a sixteen candle powder incandescent electric lamp; S_2 consists of two narrow strips of thin copper, one of which is fastened to each prong of the large tuning fork (F_1) (see Fig. 2), and each has

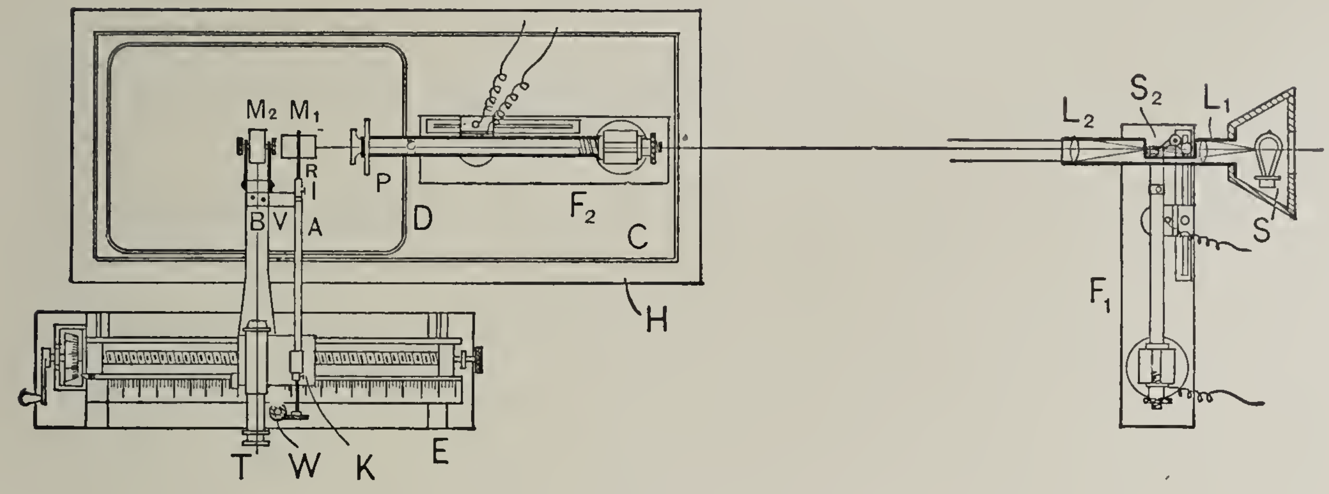

Fig. 1.

near its upper end a small hole so placed that when the fork is at rest these holes shall lie one directly behind the other in a horizontal line. F_2 is the fork which generates the ripples by means of the plate of glass (P) 2.5 by 9.7 cm. L_1 and L_2 are two small double-convex lenses. D is the vessel for holding the liquid to be experimented upon; it is a porcelain developing tray 2.3 by 29 by 36 centimeters. E is a dividing engine furnished with a screw whose pitch is 1.0328 millimeters. M_1 and M_2 are silver-on-glass mirrors. T is a telescope. To the carriage of the dividing engine is rigidly fastened a wooden arm (B) and a brass tube (A); the tube is parallel to the wooden arm and is fastened to it by means of V. To the end of B is fastened the mirror M_2 by means of brass mountings that allow it to be turned and clamped in any position desired. Through the tube A passes a brass rod (R) to which is fastened the mirror M_1. This allows M_1 to be rotated about the axis of the tube, and it is clamped in the necessary position by means of the screws I and K. W is an iron weight hung from a rod fastened to the carriage, and is intended to counterbalance the rod, mirrors, etc.

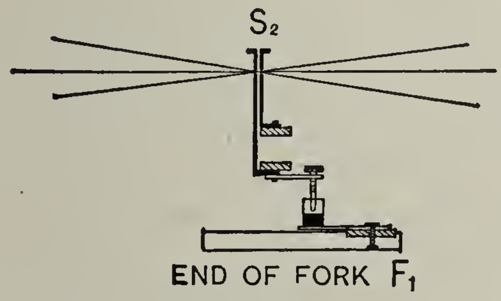

Fig. 2.

To protect the liquid as well as possible from dust and other impurities in the air, the tray D and the fork F_2 were placed in the galvanized iron box (C) which was closed by a tightly fitting lid provided with a window of good, *plane, parallel* plate glass, through which all readings were taken.

The fork F_1 is an ordinary electrically driven tuning fork provided with a mercury platinum contact maker. F_2 is also an electrically driven tuning fork. It is tuned in unison with F_1, and is driven by it by means of a shunt circuit.

By means of L_1 the light from S is focused on a point midway between the two holes in S_2; it then passes through L_2 which renders it parallel, then it is reflected by M_1 to the surface of the liquid, from this it is reflected to M_2, and from M_2 into the telescope T. If now the forks are started and the telescope is focused for the light reflected from the crests of the waves, the entire field of the telescope, which is dimly illuminated, will be crossed by a series of parallel bright lines which correspond to the crests of the waves. These lines appear stationary, and the distance between them is approximately one-half the wave-length, since each wave is seen in two positions, once when the prongs of F_1 pass through their positions of equilibrium and are approaching one another; and again when they are separating. The crests seen in one position do not lie exactly midway between those seen in the other.

The plan of operation is to set the spider line of the telescope on one of these lines, read the position of the carriage, then move it along several centimeters, and set on another line of the same series. The difference of these two readings divided by the number of waves passed over gives the wave-length.

SOURCES OF ERROR AND ADJUSTMENTS.

The first objection that presents itself is that, as the carriage is moved, the mirrors will not remain parallel to their original positions. Owing to the length of the arms supporting the mirrors (30 cm.) and to the distance of the liquid below the mirrors (45 cm.), any slight displacement of the mirrors will produce a great error in the determination of the wave-length. These errors may be introduced either by irregularities in the ways of the engine, or by a motion of the arms with respect to the carriage. The latter is simply a matter of rigid connections and can be easily remedied, so we shall first consider the former.

To test the accuracy of the ways, the rotation of the carriage was tested about three rectangular axes—one along the screw, another horizontal and perpendicular to the screw, and the third vertical. To do this, a mirror was mounted on the carriage perpendicular to the length of the screw, and a telescope (placed about ten feet from it) was focused on the image in the mirror of a vertical scale fixed to the telescope. If the carriage rotates about a horizontal axis perpendicular to the screw, the image of the scale will be displaced vertically with respect to the spider lines of the telescope. The carriage was moved several times over the entire length of the ways, but no displacement of the scale was noticed, except what might be due to changing my position with respect to the stand on which the telescope was supported.

The rotations about the other axes were tested in a similar manner and with the same result. This proved that the irregularities of the ways are at most very slight.

To test the rigidity of the arms and mirror clamps, the carriage was screwed along until the spider line in the telescope on the carriage coincided with a well defined crest. It was then slipped back, leaving the nut in place on the screw, and again moved up by hand to the nut. The line again coincided with the crest. This proved that moving the carriage by hand produced no effect on the relative positions of the carriage and its appurtenances. The carriage was then again slid from the nut, and, while held with one hand it was struck several sharp, horizontal taps with a block of soft wood. It was then moved back against the nut. If the telescope and mirrors are clamped tightly the line again coincides with the crest. This proves that slight jars of the screw will introduce no error into the results if the telescope and mirrors are well clamped.

Having settled this point, the other adjustments, which will now be described, were made; and then the ways were again tested by a method which will be described further on, and which is much more delicate than the one first used.

Leaving aside the accuracy of the screw and the motion of the carriage, there are six fundamental adjustments to be made.

1. The carbon filament and the axes of the lenses L_1 and L_2 must lie in a straight line.

2. The beam of light must be composed of parallel light and

3. It must be horizontal before reflection by M_1;

4. The ways of the engine must be horizontal and at such a height that the beam of parallel light may strike the mirror M_1, and

5. They must be parallel to the beam of light ;

6. The fork F_2 must be parallel to the ways of the engine. This is easily adjusted by means of a plumb line hung from the rod R.

1. The lenses L_1 and L_2 are placed in a tin tube against diaphragms perpendicular to the axis of the tube. This makes their axes practically coincide. The lamp S is then adjusted so that the light (from the filament that is to be used) after passing through L_1 covers the centre of L_2. Then the filament and the axes of L_1 and L_2 are in a straight line.

2. The lense L_1 is moved until the light is focused on S_2 and then L_2 is moved until on reflecting the beam of light back along its original path, it appears to converge to the hole in S_2, through which it originally came. The position of L_2 is then adjusted until the light is as nearly parallel as one can detect with a small telescope focused for infinity.

3. The entire system L_2, L_1 and S, is then inclined by leveling screws until the beam of light is as nearly horizontal as can be detected by means of a water level two meters long.

4. The engine is placed on a slab of slate mounted on leveling screws and at the desired height. The horizontality of the ways is tested by means of a delicate spirit level.

5. If two strips of paper are pasted on M_1 so that their edges touch opposite sides of the part of M_1 that is covered by the beam of parallel light; and if the engine is moved until the light remains on this spot while the carriage is moved over the entire length of the ways (about 50 cm.) we may be sure that the ways are quite approximately parallel to the beam of parallel light.

If water is now placed in the tray, if the mirrors and the telescope are turned so that the light is thrown down the tube of the telescope, and if cross-threads are stretched across L_2, a dark cross will be seen in the field of the telescope when it is focused for a point about one meter below the surface of the water (which is the focal length used throughout this work). The cross-threads in the telescope are made to coincide with this cross, and the carriage is moved along the ways.

If the ways are accurate, and if the other adjustments have been well made, these crosses will coincide throughout the length of the screw. But, owing to slight irregularities in the ways, it is found that the crosses are relatively very slightly shifted, though from scale reading 15 to scale reading 29 they remained coincident, except from 22.6 to 24.3 where one bearing of the carriage rests upon a slight dent in one of the ways. Hence, all settings were taken between 16.5 and 22, and between 25 and 27. In these regions I could detect not the slightest shift; so I think the readings are entirely free from any error due to inaccuracies in the ways. Any error in the other adjustments also causes a shift in the positions of the crosses, but this is uniform throughout the length of the ways, and so can easily be distinguished from the nonuniform displacement due to faults in the ways.

Extraneous Disturbances.

Having disposed of these difficulties we come to another that gave great trouble. This is the disturbance of the liquid by extraneous vibrations. After trying many plans I finally placed the box containing the tray of water and the fork F_2 upon an iron slab (H) weighing almost 150 pounds, and supported by long steel springs hung from a joist whose ends rested in the walls of the building but which was free from the floor above. It was hung so that when weighted for work it was about three or four centimeters above the top of a brick pier, and the intermediate space between it and the pier was loosely filled with cotton batting. If the cotton is packed neither too tightly nor too loosely, this arrangement cuts out most extraneous disturbances. The dividing engine was supported on this pier. The fork which generated the ripples, and the vessel of water were each supported on pieces of rubber tubing so that the water might not be disturbed by any slight vibration of the stand of the fork.

(*To be concluded.*)

MINOR CONTRIBUTION.

A NEW METHOD OF DETERMINING THE SPECIFIC HEATS OF LIQUIDS.

BY ROBERT L. LITCH.

THE following methods have been used for determining the specific heats of liquids: (1). Method of cooling. (2). Method of mixtures. (3). [1] Method of Thomson. (4). Electrical methods as proposed by Jamin[2] and Anmary, Weber, Pfaundler, Dieterici.[3]

The errors to which these methods are liable are too well known to need comment. I propose in this paper to outline a new method which shall involve not only the virtues lying in the so-called law of Joule but also the principle which is the foundation of the "Waterman[4] Calorimeter for the method of mixtures," viz., the maintaining of the calorimeter cup at a constant temperature throughout the experiment. By this principle the errors due to the uncertainty as to what corrections must be made for "Water Equivalent" and "Radiation correction" are avoided.

I. THEORY OF THE METHOD.

Witnin two vessels, arranged one above the other, the liquid whose specific heat is desired is placed. This particular arrangement enables the operator to easily transfer liquid from one vessel to the other. The assumption is made that the temperature of the two are different, that of the upper vessel being the lower. Though theory shows it is not necessary, yet practice has made it appear advisable to make the temperature of the cooler liquid as near 0° C. as possible. The temperature of the other is that of the room.

In the lower vessel a coil of wire is also placed. A current of known strength is passed through the coil and the temperature of the liquid is, in consequence, raised. Enough liquid is dropped into the lower vessel from the upper to maintain the temperature of the liquid in the lower constant during the operation.

Then it is evident that

$$H = sm(T - T_0)$$

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[1] Pogg. Ann. 142, pp. 337.

[2] Comptes Rendus, LXX., pp. 661.

[3] Wied. Ann. 33, pp. 417.

[4] Phys. Rev., Vol. IV., No. 21.

where H = heat developed
s = specific heat
m = mass of liquid dropped
$T-T_0$ = difference in temperature between that of the cooler liquid and the room.

If we substitute this value in the expression for Joule's Law, we have

$$\text{(I)}\quad s=\frac{rc^2t}{Jm(T-T_0)}$$

where r, c, t and J have the usual meaning.

If we take two such calorimeters containing different liquids and allow the same current to pass through them it is evident that

$$\text{(II)}\quad \frac{s_1}{s_2}=\frac{m_2r_1(T-T_{02})}{m_1r_2(T-T_{01})}$$

where the subscripts refer to the two calorimeters.

II. Apparatus.

My apparatus is constructed in many respects on the lines of the "Waterman Calorimeter." The cooler and air-thermometer are direct copies of the corresponding parts in that apparatus. The test-tube, however, is made of very thin glass and not of silver. It passes through a cork and with it completely closes the bulb of an air-thermometer. Inside the tube is a coil of platinum wire whose ends pierce the sides of the tube and are soldered to large copper wires. These wires serve as "leads" and run up along the sides of the tube, entering the open air through the cork at the mouth of the bulb.

The other pieces of apparatus found necessary are: (1) Two thermometers, one graduated to $\frac{1}{20}$ degree, so arranged in the cooler that the liquid flows over its bulb just at the moment of passing from cooler to test tube, and another graduated to $\frac{1}{5}$ degree for measuring the room temperature; (2) a copper voltameter; (3) a D'Arsonval galvanometer for the measurement of resistance by the *fall of potential* method; (4) storage batteries; (5) miscellaneous apparatus, such as variable resistance-boxes, direct reading ammeter, keys, etc.

III. Practice of the Method.

The operations necessary are: (*a*) determination of current; (*b*) determination of the amount of liquid dropped; (*c*) measurement of time; (*d*) measurement of resistance of coil; (*e*) observation of the temperatures of the liquid in the two vessels; (*f*) substitution in the formula

$$s=\frac{c^2rt}{Jm(T-T_0)}$$

In my experiments I have used an ordinary watch. This has been possible because by using small currents the experiment could be continued from five to ten minutes, and hence, the total number of seconds being large, an error of one second would introduce no perceptible change in the results. With a spring contact key, such as I have used, it is possible to turn the current on and off so near the second intended that the sum of the errors introduced by these two operations is undoubtedly much less than a second.

The measurement of resistance is the most important of all the operations. It is necessary to measure this during the experiment itself. It is not sufficient to know it at zero degrees and its temperature coefficient, because the exact temperature of the coil cannot be determined while the operation is going on. To regard it as being the same as the liquid is erroneous, for it is easy to see that, were it so, no flow of heat could take place from the coil to the liquid. To overcome this difficulty I have made use of the *fall of potential* method for this observation. This has the virtue of being easy of application and, for the small resistances I have used, of great accuracy. For my coil of known resistance, because of its slight change resistance due to a rise in temperature, large German-silver wire (No. 16, B. W. G.) was employed. This arrangement has been found very satisfactory.

IV. Results Obtained.

Owing to various reasons I have been unable to apply my method to but one substance, viz., water. Even with this I have taken no great precaution that it should be absolutely pure and hence the results are given merely to show how the method works. Rowland's statement that the specific heat of water decreases between 4° and 30° C. is seen to be followed in these results:

T	s
18.8°	.98075
19.7	.98064
21.05	.98035
21.2	.98035

These values lie between those given by Dieterici and Velten for 20°, the former obtaining .9893, the latter .9794. The method of mixtures has invariably given for this temperature values greater than unity.

V. Second Method.

The values of J used in the experiments under the first method are those due to Rowland. They are probably the best so far obtained but notwithstanding this fact there is undoubtedly a great degree of indeterminateness as to the correct value of this constant and hence it is best to eliminate it, if possible, from any method of experimentation.

A glance at equation (II) reveals the fact that we require no knowledge of (1) current, (2) time, (3) mechanical equivalent. Why these three quantities disappear from the equation appears in the following considerations. The two calorimeters necessary for the practice of this method have their coils joined in series and hence the same current flows through both. The consequence is that whatever fluctuation in the current occurs, the effect in one is as great as in the other. It follows, therefore, that at every instant the current through the first is equal to that through the second and hence the ratio of the two is vigorously unity. Similar reasoning will show why the other two quantities are eliminated.

The first method requires a knowledge of all three and in addition we are compelled to make use of the average current. Trouble arises here because when set into the formula it must be squared giving a value $[1/t\sum i]^2$ whereas we desire $1/t\sum i^2$. And while it may be shown that "For small variations of the current the mean of the squares may be equated to the square of the mean," yet any approximation it may become capable of furnishing is entirely dependent on the hypothesis that the variations are small. But as it is impossible to ensure that they shall be so, any results derived on the assumption that they are must involve an error of indeterminate magnitude.

For these reasons, then, the second method appears much more satisfactory. Up to the present time I have been unable to try it, but shall attempt to do so at a later day.

In conclusion I take this opportunity to express the obligations I feel myself under to Prof. W. F. Magie and Dr. F. A. Waterman, the latter of whom has labored with me in a most kindly way giving hints and suggestions which his work with his own calorimeter has shown him would be advantageous and which have proven so in every instance.

Physical Laboratory, Princeton University.

NEW BOOKS.

Experimental Physics. By W. A. Stone. 8vo, pp. 378. Boston, Ginn & Co., 1897.

First Principles of Natural Philosophy. By A. E. Dolbear. 8vo, pp. 318. Boston, Ginn & Co., 1897.

The recognition of laboratory exercises as an important factor in the teaching of elementary physics has led to the repetition of the old experience that a young and long neglected party, which at first claimed equality only with an older one, gradually tries to assume all the rights and leave nothing to the other. Mr. Stone seemingly is a partisan of the new régime, but not to such a degree that he expects a student to be able to deduce the laws of nature unaided. The elegant way in which the author makes the older "text-book" politely concede the precedence to the newcomer, but afterwards guide him along the proper road by well put questions and directions, will commend this book to any teacher using the laboratory method exclusively. Quantitative experiments are not undertaken until the student has learned by qualitative work something about that which he is going to measure. It is to be regretted that in the deduction of the laws so little use has been made of mathematical expressions as a simple means of combining several wordy rules, while on the other hand the introduction of the graphical method in its simplest form is a great step in advance.

The arrangement of the subject is quite original. Mechanics forms the sixth of the eight chapters of the book, the preceding ones being: Mensuration and Hydrostatics, Heat, Elasticity, Sound, Light. The reason for this change from the usual method lies probably in the attempt to make the subject as palatable as possible for the beginner, who often loses the taste for physics by being overworked with experiments in mechanics. It seems, however, very peculiar that the study of mass, force, work, motion, etc. (in this order!) is begun on page 245. Mr. Stone has had an experience of nearly ten years in teaching the subject and it is impossible for anyone who has no experience as to the practical working of the above method, to form a definite judgment on it; but it seems to me, that questions must come up in the very beginning of the course, the answer to which is reserved for the second half of the book.

The first six chapters are treated very satisfactorily, while this cannot be said of the chapter on electricity. The book is intended as well for students not preparing for college as for those who do, and it will be, especially for the former class, an unsatisfactory course in physics, that teaches them nothing of static electricity, electro-magnetic induction and all the important applications of electricity: the telegraph and telephone, dynamo-electric machinery and the electric light. It must be admitted that it is very difficult, to include these subjects in a book based on the laboratory method, but they ought not to be left out in any course on physics, however unpretentious.

Mr. Stone's book is intended primarily for schools preparing for Har vard and will be of great value for such, since it contains the experiments recommended by that university as a good preparation for physics.

Professor Dolbear's "text-book" is of an entirely different type and intended for a different class of students. It was to be expected that the author of "Matter, Ether and Motion" would "direct the attention of the student from the physics of mechanism to the physics of the molecules and help him to carry the mechanical conceptions gained by the study of visible bodies to their ultimate particles." The vortex-ring is introduced at the very beginning and its vibrations play a very important part in the explanation of thermal, optical and electrical phenomena. These very interesting speculations, though it seems to me, too advanced for a beginner, justify fully the return to the old name: "Natural Philosophy."

The very conservative standpoint of the author is best shown by the exclusive use of the English units, while the C. G. S. system has not even been mentioned. In what a sharp contrast does this stand to last year's unanimous resolution, of the American Association, demanding a reform in weights and measures and further legislation looking to the early adoption of the metric system. Unfortunately there are several misleading statements and misprints in the book that require great care in its use.

The popular character of the book taken as a whole, and the fact, that its study requires hardly any mathematical preparation, will introduce it into certain schools and courses of physics, where other text-books are only a burden to the teacher and entirely too advanced for the students.

K. E. GUTHE.

Alternating Current Phenomena. By CHARLES P. STEINMETZ. 8vo, 424 pp. New York, W. J. Johnston Company, 1897.

The development of the commercial application of electricity is a most curious incident in the history of human progress. The lightning's flash, and the reverberating roll of the thunder revealed the presence of this form of energy to primeval man, while from the far north, the pale gleams of the aurora contained a prophesy of Mr. Tesla's Wireless

Transmission, and heralded all the wonders of the vacuum tube and cold light. Twenty centuries ago the Greeks baptized electricity, but seemingly were poor sponsors, caring but little for the development of their progency, for excluding a Celestial knowledge of the compass, and a few lines in the Vedas which fanciful translators strain into a reference to incandescent lighting and the telephone, there was not a single practical application of electricity until near the middle of the present century. Indeed, a majority of us of barely middle age, withal, remember that the electrical engineering courses of the best schools of our youth, consisted in an inspection of a telegraph relay, and a study of repulsion as manifested by a long haired comrade mounted upon a insulating stool. But "mutatis, mutandis," for if the 19th century be termed "The Age of Mechanics" its last two decades form the "Era of Electricity," as during this period the development of industrial electrical appliances has caused an average annual investment of $100,000,000.00. As an urban illuminant electricity is all but universal; we talk over half a continent; Puck's girdle may almost be accomplished in forty seconds; the mention of a "horse car" provokes a smile; the energy of the greatest cataract on the globe, now diverted, is directed and delivered at pleasure over a network of conductors. To what shall this phenomenal development be ascribed? Why did the germs of electrical science lie dormant for two thousand years to sprout, in two decades with such cryptogamic rapidity? How happens it that electricity in twenty years, out-strips steam engineering of two hundred years?

It is additionally curious to note the present atavistic tendency of electrical progress. Excluding the telegraph and electro-plating, all of the original attempts at industrial application employed alternating currents. Witness the Jablochoff candle; the early permanent magnet dynamos, and the pulsations of telephony; but forsooth, because the galvanic battery warped sprouting electrical intelligence into considering that only uni-directional energy was commercial, the far more flexible and docile wave manifestation was abandoned, and electricians, imprisoning themselves with communtator segments, declared alternating currents to be dangerous, unmanagable, expensive and impracticable. But thanks to just such men as the author of alternating current phenomena—men having the ability both to perceive relationships, and to intelligently express them—old limitations are disappearing, and all electrical operations are becoming reduced to scientific unity. And just here the clear sighted perceive the true reason for the singular rapidity of electrical development. All knowledge is but an empiric record of experience, but so long as any art is but a series of scattered empiricisms, progress is impossible, and it is not until reason, ascending from the deductive to the higher levels of inductive ratiocination, aided by a scientific use of the imagination, lights the way from the known into the unknown, that real and substantial advancement is made. The mathematicians are the architects of the present electrical structure, and alternating current phenomena presents the latest and most complete exposition of what might be termed "Electrical Mathematics" for development is reciprocal, and as electric science would be impossible without mathematics, conversely electrical investigation has constructed for itself a special branch of symbolic expression.

In 1893 Mr. Steinmetz prepared the way for the present volume by presenting to the International Electrical Congress, a paper on the application of the "Algebra of Complex Quantities to Electrical Problems." While the title of the paper was alarming, it was shown that the use of plane algebra greatly facilitated and abridged electrical calculations, being almost essential to a comprehension of the more involved relationships of alternating currents, and upon the foundation thus established, the author has now elaborated his solutions until they embrace a consideration of nearly all the phenomena at present applying to electrical currents. Abandoning preconceived notions of electricity, as restricted to continuous currents, Mr. Steinmetz prepares the way to a study of his methods by three chapters, occupying some twenty pages of introductory matter in which electrical laws, so broadly generalized as to apply to all phases of this manifestation, are enumerated. In the next three chapters the three fundamental processes of calculation are explained, Graphical Representation by means of polar coördinates being placed first. Passing to symbolic methods the author next shows that, while the clearest mental picture of the reciprocal reactions of alternating currents may be formed by Graphical Representation, it is often practically impossible to get accurate results, owing to draughting limitations, thus indicating the superiority of symbolic calculation. This chapter, in connection with the various explanatory appendices, will place the student in a position to handle plane algebra as applied to electrical currents. As a purely mathematical treatise this portion of the work may seem restricted, yet the busy engineer will welcome the kernel stript of the quaternionic husk of the more voluminous works on higher mathematics, and the author is to be congratulated upon successfully exorcising the famous bug-bear of $\sqrt{-1}$. In the succeeding Topographic Method, is presented an ingenious graphical protrayal of the reactions of sine waves in inter-linked circuits, that avoids some of the incongruities exhibited in polar diagrams.

Having thus developed the general scheme of analysis, the fundamental characteristics of circuits: Admittance, conductance, susceptance, resistance, inductance and capacity are taken in order, both symbolically and graphically, and analytical methods applied to determine the effect in general of the presence of these factors and their combinations. So far the volume is occupied purely with general mathematical statements, although reinforced with adequate practical illustrations, but in Chapters IX., X. and XII. the application of these principles to transmission lines (the important problem to practicing engineers), is taken in hand in an extremely valuable manner. Logically, Chapter XI., on Foucault Currents, and a portion of Chapter X., dealing with the effect of hysteresis, appear out of place, and would seem more properly found as an introduction to the subsequent sections on alternating current machinery. Completing the investigation of the properties of transmission circuits, Mr. Steinmetz, in Chapter XIII., commences the study of alternating current machinery with an extensive investigation of the transformer. The induction motor receives a fair share of attention, while synchronous motors and generators are treated in extenso. Communtator motors and reaction machinery are mentioned seemingly for the sake of completeness, as little encouragement is offered for their practical utilization. The last third of the volume is occupied with wave distortion, the effect of harmonies and a consideration of general polyphase systems, including an investigation into the required amount of conducting material. Here again some illogicality in arrangement is apparent.

Beyond question, Mr. Steinmetz has carried mathematical analysis, as applied to electricity, further than any other American engineer; so far, indeed, that time is the only criterion with which to test the work, for at present his methods extend over the confines of practice into the domain of pure theory. But the author has given the profession an edged tool, so keen withal, that a wise man may carve himself a fortune or the fool kill himself therewith, and so a reference to the aphorism regarding cutting edges is apposite.

There is a wealth of information in the book, profusely illustrated with supposititious practical examples, for which the author deserves the gratitude of all electricians; but it is so concentrated that those of us who are not mathematical ostriches are likely to have an attack of intellectual dyspepsia. To Lord Kelvin or Mr. Heaviside, alternating current phenomena would probably be only light literature, to accompany black coffee and a post-prandial cigar, but for the rank and file of electrical engineers, the food is too concentrated and should be peptonized with additional explanation. Typographically the volume is in accord with the well-known work of the publishers and the absence of errors in matter of so symbolic a character is notable, but in the diagrams it is again a pity that so much compression is employed as to make the subscript letters microscopically illegible.

Prophets are without honor, not only in their own country, but as well in their day and generation, and Mr. Steinmetz will have to wait the fullness of time to receive the appreciation his work unquestionably deserves, but if the present rapidity of electrical development continues, his reward will not be a posthumous one.

A. V. Abbott.

The Polarizing Photo-Chronograph. By Albert Cushing Crehore, Ph.D. and George Owen Squier, Ph.D. Pp. vii+150. New York, John Wiley & Sons, 1897.

This volume consists of a reprint of the three papers upon the measurement of the velocity of projectiles made by Messrs. Crehore and Squier at the United States Artillery School, Fort Monroe, Virginia, and originally published in the *Journal of the United States Artillery*. The authors have added a brief introduction of five pages, and have published Dr. Crehore's paper describing the origin and development of his method of recording variable current curves[1] as an appendix to the volume. The placing of this paper in an appendix instead of making it the introductory number of the series may be regarded as something of a deviation from the plan of presentation outlined in the preface; since it was the first in the order of time and likewise first in the history of the development of the method. The arrangement adopted is, however, justified in the fact that the method underwent very radical changes before its application to the measurement of the motion of projectiles.

The work of Crehore and Squier affords a striking illustration of the way in which, from time to time, phenomena in physics which seem at first sight most remote from any possibility of utilitarian interest, are put to practical use. The rotation of the plane of the polarization of light in the magnetic field would surely have been considered such a phenomenon; and yet we suddenly find it utilized in the furtherance of an important art. A later communication from the same authors[2] foreshadows its application, indeed, in the much broader and more important field of high speed telegraphy.

The experiments of which the book treats are clearly described in terms which will render them intelligible and interesting both to the man of science and to the military engineer. The interest of the physicist in the memoirs consists more particularly in the possibilities which they suggest of further applications of the method to the registration of short intervals of time than in the results which have been obtained upon the flight of projectiles. To the student of the art of practical gunnery, on the other hand, the data given will, on account of their definite and accurate character, be of the greatest service.

E. L. Nichols.

[1] Physical Review, Vol. II, p. 122.

[2] Transactions of the American Institute of Electrical Engineers, April, 1897.

Contributions to the Analysis of the Sensations. By Ernst Mach. Translated by C. M. Williams. 8vo, pp. viii + 208. Chicago, The Open Court Publishing Co., 1897. (*Received.*)

The Scientific Papers of John Couch Adams. Edited by William Grylls Adams, with a Memoir by J. W. L. Glaisher. pp. liv and 502. Cambridge, at the University Press, 1896. (*Received.*)

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THE

PHYSICAL REVIEW.

THE DISCHARGE OF ELECTRIFIED BODIES BY THE X-RAYS. I.

BY CLEMENT D. CHILD.

SINCE the first article published by Roentgen announcing the discovery of the X-rays, there have been only a few points established which give further knowledge concerning their properties. Of these the discovery that X-rays discharge an electrified body is, perhaps, the most important. Some contradictions concerning this phenomenon led me to study it, in order to determine, if possible, the effect on the rate of discharge produced by a variation in the density of the air surrounding the electrified body. Before giving my own work, it will not be out of place to review briefly the articles on this subject which have already been published.

Resumé of Articles Published.—Lenard had found that the cathode rays would discharge electrified bodies, and Roentgen[1] was aware that X-rays possessed this same property at the time of the first announcement of his discovery. The first published accounts of this effect, however, were given almost simultaneously by Benoist and Hurmuzescu[2] under date of February 3, 1896, and by J. J. Thomson,[3] on February 7.

Question of Final Charge.—Concerning the fact of discharge there has never been any question, but very contradictory results were obtained by the first experimenters as to the complete discharge of a body, and as to the charging of a body originally unelectrified. Benoist and Hurmusezcu[1] state that the discharge is complete, and that it is more rapid if it be of the negative sign. Borgman and Gerchun[2] state that a zinc plate loses a positive charge rapidly and then becomes negatively charged to a certain fixed value; but if charged negatively at first, the charge is dissipated far more slowly and stops at the same value as before.

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[1] Sitz. der Wuerzb., phys.-med. Ges., March, '96.

[2] C. R., 122, p. 235.

[3] Lond. Elec., 36, p. 491.

Righi[3] says that rays act upon non-conductors as well as conductors, and that the ultimate charge depends upon the kind of metal. Perrin[4] states that there is no final charge greater than $\frac{1}{20}$ of a volt. Minchin[5] states that some metals become positively charged and others negatively, when X-rays strike upon them. The order in the list which he gives agrees very closely with that in the list of metals given for contact potential differences.

Apparently, little thought was given in these experiments to the substance surrounding the metal from which the discharge took place. More attention has been given by later experimenters to this point, and more consistent results have been obtained. Murray[6] found that when X-rays passed between zinc and tin-foil the metals took on final charges corresponding to those they would have if connected by acidulated water. Perrin,[7] and Lord Kelvin, Beattie, and Smolan[8] found the same thing to be true in the case of several metals. The latter are of the opinion that there is no variation in the final potential due to variation in the intensity of the rays. Murray states that the final potential does depend somewhat upon the intensity of the rays striking the plate. Righi[9] found that the pressure of the air at which the charging effect of the rays is greatest is lower than atmospheric pressure, and higher than the corresponding air pressure for ultra-violet light.

Measurement of Discharge with a Galvanometer.—As has been stated, the later results indicate that the air through which the X-rays are passing acts like acidulated water. This idea led Minchin[1] to endeavor to detect, by means of a galvanometer, the presence of a current when discharge is caused by X-rays. Having found the resistance in megohms between the plates where the discharge took place, he computed the current that should flow, and though his galvanometer was abundantly sensitive, he was unable to detect any sign of such a current. However, he made the assumption in this work that the current should obey Ohm's law, and, as will be shown later, this is an incorrect assumption. Whether this would entirely invalidate Minchin's work or not, is not certain.

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[1] C. R., 122, pp. 235, 779 993.

[2] C. R., 122, p. 378.

[3] C. R., 122, pp. 376 and 601.

[4] L'Ecl. El., 7, p. 525.

[5] London Elec., 36, p. 736.

[6] Proc. Roy. Soc. Lond., 59, p. 333.

[7] C. R., 124, p. 496.

[8] Nature, 55, p. 344.

[9] C. R., 122, p. 878.

Perrin found quite the opposite result from that found by Minchin. He states[2] that when X-rays were allowed to pass between a copper and a zinc plate, a current of 7×10^{-9} ampere was produced as measured by a galvanometer. The area of each plate was 100 cm.^2 and there was no electromotive force except that caused by the X-rays between the copper and zinc plates.

Discharge by Roentgenized Air.—It was soon found that it was not necessary for the X-rays themselves to strike the electrified body in order that discharge should take place. It is sufficient if the body is in an atmosphere through which the X-rays have passed. Various experiments to show this were given by Villari[3] and others.

Discharge in Different Gases.—The discharging effect in different gases has been studied by Roentgen, J. J. Thomson, and others. Roentgen, in the article to which reference has already been made, states that the action is somewhat slower in hydrogen than in air. Righi[4] states that the rate of discharge varies as the density of the gas in which the discharge occurs.

Benoist and Hurmuzescu[5] give the rates of discharge in air, CO_2, and H, and find that these rates are approximately proportional to the square roots of the densities.

Thomson and McClelland[6] give the relative rates of discharge in several different gases. The data there given do not agree with the law stated by Benoist and Hurmuzescu. They conclude from their table "that the electrical conductivity of the heavier gases, when exposed to the Roentgen rays, is greater than that of the lighter gases;

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[1] Lond. Elec., 38, p. 789.

[2] C. R., 124, p. 497.

[3] Rend. Acc. Linc. 5, p. 35.

[4] Nuovo Cim., 3, p. 177.

[5] C. R., 122, p. 926.

[6] Proc. Camb. Phil. Soc. 9, p. 128.

the order of the electrical conductivity does not, however, exactly correspond with the order of molecular weight. A very marked feature of the table is the high conductivity of the halogens. The high conductivity of mercury vapor is very remarkable, for this gas allows the ordinary electrical discharge to pass through only with great difficulty, whereas under the influence of the Roentgen rays it is one of the best conductors among the gases investigated."

Thomson and Rutherford[1] found that in the case of hydrogen the discharge was smaller than through air when small E.M.F.'s were used, and larger when large E.M.F.'s were used.

Discharge with Different Densities of the Gas.—The effect on the rate of discharge caused by varying the pressure of the surrounding gas has been studied by Perrin,[2] Villari,[3] Benoist and Hurmuzescu,[4] and Thomson and McClelland.[5] Perrin and Villari state that the rate of discharge is proportional to the density of the surrounding gas, but no data are given by them. The others reached the conclusion that the rate of discharge varies as the square root of the density. Benoist and Hurmuzescu determined this rate by noting the time required for the leaves of an electroscope to fall from a given initial to a given final position. The potential to which the electroscope was charged is not given, but was presumably quite large.

Thomson and Rutherford studied this effect in several different gases. They used an electrometer and noted the time that it took for the potential of the charged body to fall a definite amount. The data given by them for air and coal gas agree fairly well with the law that the rate of discharge is proportional to the square root of the density ; but if their data in the case of sulphuretted hydrogen, and especially in the case of sulphur dioxide, be plotted, the curves seem to show a systematic deviation from this law.

Roentgen makes the statement that the discharge was found to be much slower at quite low pressures than at atmospheric pressure. Righi[6] states that under ordinary conditions the discharging effect of X-rays diminishes as the density of the air is diminished, while the effect due to ultra-violet light increases. The results of my own ex-

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[1] Phil. Mag., 42, p. 392.

[2] C. R., 123, p. 878.

[3] Nuovo Cim., Sept., 1896.

[4] C. R. 122, p. 926, and 123, p. 1265.

[5] Proc. Camb. Phil. Soc., 9, p. 128.

[6] C. R., 122, p. 878.

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periments on the effect produced by varying the density of the gas will be given elsewhere in this article.

Discharge at Different Temperatures.—Perrin states that the rate of discharge is proportional to the absolute temperature, but no data are given in support of this view. Thomson and McClelland find that as the gas is heated its conductivity reaches a maximum and then diminishes.

Discharge with Different Potential Differences.—The rate of discharge increases as the potential of the charged body increases. However, the increase does not obey Ohm's law. This effect has been studied by Thomson and Rutherford[1]. For low potentials the rate of discharge is found to obey Ohm's law fairly well, but as the potential is increased the current tends to approach a limiting value.

Discharge with Different Distances between the Plates.—Thomson and Rutherford also studied the effect upon the rate of discharge caused by varying the distance between the charged body and a grounded body placed near to it. Contrary to what might have been expected, it was found that the nearer the plates were together, the less rapid the discharge. This result is also confirmed by Righi[2].

Discharge from Different Metals.—Benoist and Hurmuzescu[3] state that the time taken for a given discharge varies with the metal from which the discharge takes place, and that the rate of discharge is inversely proportional to the transparency of the metal for X-rays.

Discharge through Solid Dielectrics.—There has been some discussion as to whether discharge takes place in the case of a solid non-conductor. J. J. Thomson's first statement was that non-conductors behave as conductors when acted upon by X-rays. This statement, however, has been questioned by other experimenters. Villari[4] states that there is no discharge through paraffin, even when there is only a thin layer of it. A more correct statement of the case seems to be given by Thomson[5] He states that when a charged body surrounded by paraffin is placed in the path of the X-rays, there is at first a large leakage of electricity into the paraffin, but that this action soon stops. If then the charged body be grounded while the X-rays are turned off, upon turning them on again there is a large discharge of electricity out of the paraffin. This electricity may be held by the paraffin a long time, if it is not acted upon by the X-rays. Lord Kelvin, Beattie, and Smolin[1] came to the conclusion that paraffin and glass are not rendered sensibly conducting by the X-rays. Burke[2] came to the same conclusion. Righi[3] states that solid and liquid non-conductors do not become conductors; in petroleum and vaseline the discharge soon stopped, but in sulphuric ether it was very much increased. In the latter part of this article an experiment is described bearing upon this point.

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[1] Phil. Mag., 42, p. 392.

[2] Rend. Acc. dei Lincei., 5, p. 342.

[3] C. R., 122, p. 779.

[4] Nuovo Cim., May, 1896.

[5] Nature, 54, p. 302.

Electrification of the Air.—Lord Kelvin, Beattie, and Smolan[4] state that the air under the action of the X-rays, and in the neighborhood of a charged body, becomes electrified. Rutherford[5] also found this to be true. He states that the amount of electrification is proportional to the conductivity of the gas and to the absorption of the rays by the gas; the amount of electrification does not depend upon the amount of dust in the air, nor is it affected materially by the metal from which the discharge takes place. It is greatly affected, however, by the kind of gas through which the discharge occurs. There appears to be some conflict regarding the character of the charge given to the air. The first writers state that it is always negative. Rutherford states that the charge in the air is always opposite in sign to that of the metal from which the discharge takes place. It is difficult to see why either of these statements should always be true, and it would seem that more experimental data upon this point are needed.

Law of Inverse Squares.—It has been found by Thomson and McClelland, and by others, that the rate of discharge varies inversely as the square of the distance from the tube giving off the rays.

Allied Phenomena.—Several phenomena are known which are more or less allied with the discharging effect of X-rays. Villari[6] states that gases which are in this Roentgenized condition have either a greater thermal capacity or a greater thermal conductivity. This he says is shown by heating a platinum wire to red heat by a current. When the X-rays are turned upon the wire, it becomes perceptibly cooler. Upon trying this experiment in a crude manner myself, I was unable to detect any such effect.

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[1] Nature, 55, pp. 472, 498.

[2] Lond. Elec., 37, p. 374.

[3] Rend. Acc. Linc., 5, p. 342.

[4] Nature, 55, p. 199.

[5] Phil. Mag., 43, p. 241.

[6] C. R., 123, p. 398.

Villari[1] also states that Roentgenized air is in a condition opposite to that which is produced when ozone is formed; that the two neutralize each other in their effects; and that on sending Roentgenized air through an ozonizing apparatus, its peculiar properties are destroyed.

Attention has also been called to the fact that gases may be given the ability to discharge electrified bodies by other means than the X-rays. Thus Villari[2] states that the air acquires this property when sparks pass through it, and Garbasso[3] states that discharge takes place when the products of combustion strike a charged body. A discussion of these phenomena will be found in Wiedemann's Electricität IV., p. 870.

Righi[4] states that the discharge takes place along lines of force.

Frankland[5] thinks that if ionization occurs, there should be a change in the rotatory power in the case of optically active substances. He did not, however, find such to be the case.

Discharge by Becquerel Rays.—Becquerel[6] has found that the invisible rays emitted by uranium have the same power of discharging electrified bodies. He finds that the effect is in every way similar to the discharge produced by X-rays. Among other things he finds that the rate of discharge is proportional to the square root of the density of the surrounding gas.

Discharge by X-rays Compared with that by Ultra-violet Light.—To compare the discharging effect of X-rays with that of ultra-violet light is, of course, one of the first things to suggest itself, but the differences in the two effects are of such a radical character that it seems scarcely worth while to go into detailed comparison.[7] The ultra-violet light discharges negative electricity only, while the X-rays act alike on positive and on negative. Ultra-violet light produces no effect except when it shines on the body to be discharged. In the case of the X-rays, however, it is only necessary that the air through which the rays have passed should strike the charged body.

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[1] C. R., 124, p. 558, and elsewhere.

[2] C. R., 123, p. 598.

[3] Nuovo Cim., 4, p. 24.

[4] C. R., 123, p. 399.

[5] Nature, 53, p. 356.

[6] C. R., 124, p. 483.

[7] See article by Merritt on "The Influence of Light upon the Discharge of Electrified Bodies." Science, 4, pp. 853 and 890.

With ultra-violet light the kind of metal and the character of the surface have a great effect upon the rate of discharge ; with X-rays these have almost no effect.

With the ultra-violet light there is a disintegration of the surface from which the discharge takes place. This phenomenon has not been shown to accompany discharge by means of the X-rays. I am not aware, however, that any attempt had been made to find an effect due to X-rays similar to that observed by Righi, showing that a charged plate is driven away from ultra-violet light, or to that of Lenard and Wolf, showing the roughening of a surface from which discharge takes place.

Experiments have been made showing that an effect is produced on the formation of fog, when steam is ready to condense, both by the ultra-violet light and by the X-rays ; but there would appear to be a radical difference between the effects in the two cases. Lenard and Wolf find that the ultra-violet light has no effect upon the formation of fog, unless the action takes place in the neighborhood of a charged body so that there may be dust particles driven off from it. Wilson[1] and Richardz[2] do not mention the necessity of a charged body when performing the similar experiment with X-rays, but think that the action is due to some effect upon the gas itself.

Since it has not been found possible to polarize X-rays, no phenomenon has been observed with them similar to the effect observed when using ultra-violet light. However, Heydweiler[3] states that he found the X-rays more effective in discharging an elecrified body when the incidence was nearly grazing. If this should prove to be true it would be an important similarity between the two classes of phenomena.

It has been found that a strong magnetic field has a great influence on the amount of discharge caused by the ultra-violet light. I am not aware that any experiments have been undertaken for the purpose of showing a similar effect in the case of the X-rays.

It has been found that if polarized light is used, the greatest discharge takes place when the plane of polarization is such that there is the greatest absorption at the charged surface ; and in general the discharging effect is greatest when there is the most absorption at the surface. But in the case of the X-rays the rate of discharge seems to depend more on the absorption of the rays by the gas surrounding the body, than upon the absorption by the substance from which the discharge takes place. Rutherford[1] states that there is a very great difference in the conductivity of different gases, and that this conductivity is proportional to the absorption of the rays. It would not appear from his experiments that the character of the metal affected the rate of discharge. However, Benoist and Hurmuzescu[2] find that the amount of discharge depends somewhat on the character of the metal used, and they state that the metals which discharge best are those which absorb the X-rays most. If this is confirmed by further experiment it will show that the action of the ultra-violet light is more similar to that of the X-rays in this respect than would now appear.

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[1] Roy. Soc. Lond., 59, p. 276.

[2] Wied. Ann., 59, p. 594.

[3] Chem. Zeitung, 53, p. 521.

According to Stoletow the rate of discharge becomes smaller, in the case of ultra-violet light, as the distance between the plates is made greater. According to Thomson and Rutherford the rate of discharge in the case of the X-rays, under the same circumstances, becomes larger.

There are, however, a few points of similarity in the two effects : In both the discharge does not increase as rapidly as the potential of the charged body, when the latter is varied. It would appear that currents may be produced by X-rays similar to the photo-electric currents of Stoletow. If the metals be arranged according to their tendency to acquire a positive or negative charge, the order in the two cases will be approximately the same, and will correspond to a series for contact difference of potential. It has been found by Cave,[3] Borgman and Gerchun,[4] Sella and Majorana,[5] Swyngedauw,[6] and Guggenheimer[7] that the effect on the sparking distance is nearly the same in the two cases. The effect due to changing the density of the gas will be considered in connection with my own work.

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[1] Phil. Mag., 43, p. 241.

[2] C. R., 122, p. 779.

[3] Lond. Elec., Feb. 21, '96.

[4] C. R., 122, p. 378.

[5] Rend. Acc. Linc., 5, pp. 323 and 389.

[6] C. R., 122, p. 374.

[7] C. R., 124, p. 259.

Theory.—Most experimenters are agreed that the effect is due to the fact that the gas becomes a conductor under the influence of the rays. The most probable explanation seems to be that given by J. J. Thomson, that the gas is in some way dissociated. This would explain all the phenomena much better than any other theory which has at yet been proposed. This theory will be discussed more in detail in the latter part of this article.

Experimental Work.

Description of Apparatus Used.—My own work has been devoted chiefly to the study of the relation between the rate of discharge and the density of the surrounding gas. The apparatus used in the present work is shown in the accompanying diagram. The zinc plate, *A*, was connected to two quadrants of the electrometer, *E*, and placed inside a zinc box, *B*, from which it was insulated.

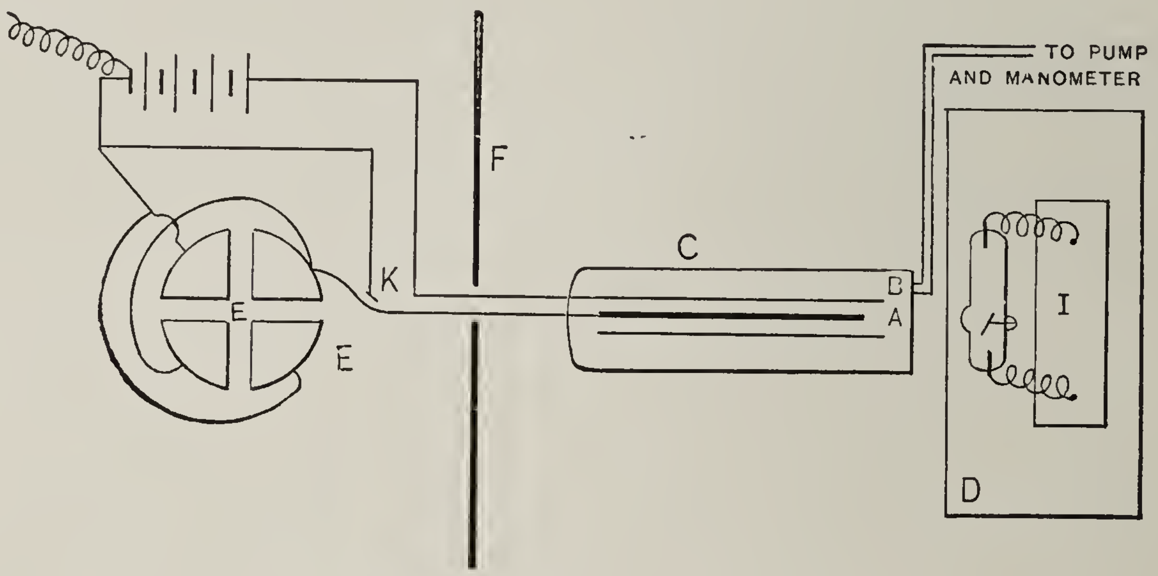

Fig. 1.

Both the plate and the box were carefully cleaned. One end of the box was partially open so that the X-rays could strike the plate at grazing incidence. The box and the plate were placed within a bell jar, *C*, the rays entering at the end of the jar. Over this end a thin piece of board was sealed. This had been soaked in paraffin and was found to be sufficiently air-tight. It possessed the advantage of being almost entirely transparent to X-rays, much more so than aluminum. It could be removed from the jar and the metal removed or its condition changed. The bell jar was connected to an air pump and to a manometer.

In most of the experiments the induction coil was operated by a magnetic interrupter. The coil, *I*, was placed within a metal box, *D*, which had an opening for the rays to pass through. This opening was covered with wire gauze to screen off electrostatic induction.

Care was taken to avoid ordinary electrical leakage. The electrometer and its connections were protected from the action of the X-rays by the sheet of lead, *F*, and they were also protected from the action of the air through which the rays had passed. The zinc plate which was used with most of the experiments had an area of 40 cm.^{2} The sides of the box were 2 cm. from the plate.

The tube used was a Bowdoin tube and had previously had much use. It appeared to be more constant in its action than other tubes tried.

Method of Taking Observations.—The method first employed was to ground the plate for an instant, and after insulating it to allow the rays to act for a given length of time, usually six seconds, between the plate and the box, the box being kept charged to a given potential. It was found that ordinary electrical leakage could best be avoided in this way; the result was the same, of course, as discharging the plate to the box, and will be spoken of as such in what follows.

Several observations were made in each case and the average taken. The difference of potential between the plate and the box did not, of course, remain the same through the discharge; but by taking an average between the initial and the final difference of potential, a fairly satisfactory result could be obtained. The work is not capable of sufficient accuracy to warrant any more elaborate correction, for it is very difficult to keep the action of a Roentgen tube perfectly constant.

Discharge with Different Densities of Gas.—It has already been stated that the relation between the rate of discharge and the density of the surrounding gas has been found to be different under different circumstances. The potential at which the discharge took place had the greatest effect upon this relation. If a curve be plotted with pressures of gas as abscissæ, and rates of discharge as ordinates, the curve will be found to take different forms as the potential is varied. The accompanying tables gives data showing the rates of discharge

Table I.

Initial Difference of Potential in Volts.		6	11	20	35
25 mm.	Fall of Potential in Scale Readings.	1.3	1.2	1.6	1.7
		1.5	1.3	1.8	1.8
		1.7	1.4	1.3	1.6
		1.8	1.5	1.6	1.7
	Average Fall.	1.6	1.4	1.6	1.7
	Average Fall in Volts.	.8	.7	.8	.85
75 mm.	Fall of Potential in Scale Readings.	3.2	3.1	3.2	3.2
		3.1	3.4	3.4	3.1
		2.8	3.4	2.6	3.5
		2.7	3.1	2.9	3.3
	Average Fall.	3.	3.2	3.	3.4
	Average Fall in Volts.	1.4	1.47	1.4	1.55
150 mm.	Fall of Potential in Scale Readings.	3.1	4.	4.	5.
		3.1	4.1	4.8	4.5
		3.3	3.9	4.4	5.1
		3.7	4.2	3.8	4.7
	Average Fall.	3.3	4.	4.3	4.8
	Average Fall in Volts.	1.5	1.8	1.95	2.15
250 mm.	Fall of Potential in Scale Readings.	3.7	4.8	6.	6.5
		3.9	5.2	4.8	6.
		3.9	4.6	5.6	6.
		3.5	4.3	5.2	6.2
	Average Fall.	3.6	4.8	5.4	6.2
	Average Fall in Volts.	1.65	2.15	2.4	2.77
500 mm.	Fall of Potential in Scale Readings.	2.6	4.2	6.2	7.5
		3.1	4.1	5.5	8.
		2.8	3.8	6.	7.8
		2.8	4.3	5.	8.
	Average Fall.	2.8	4.1	5.9	7.8
	Average Fall in Volts.	1.3	1.87	2.65	3.3
760 mm.	Fall of Potential in Scale Readings.	2.6	3.8	6.3	8.5
		2.6	3.4	6.4	10.
		2.8	3.9	6.8	9.8
		2.8	4.	6.2	9.4
	Average Fall.	2.6	3.9	6.4	9.4
	Average Fall in Volts.	1.2	1.8	2.8	3.88

at different potentials and at different pressures, and from this table curves are plotted in Fig. 2 for initial potentials of 6, 11, 20, and

35 volts. There was a condenser having a capacity of .002 microfarads in multiple with the plate at this time. The data are given in full in order that some idea may be had of the accuracy attained.

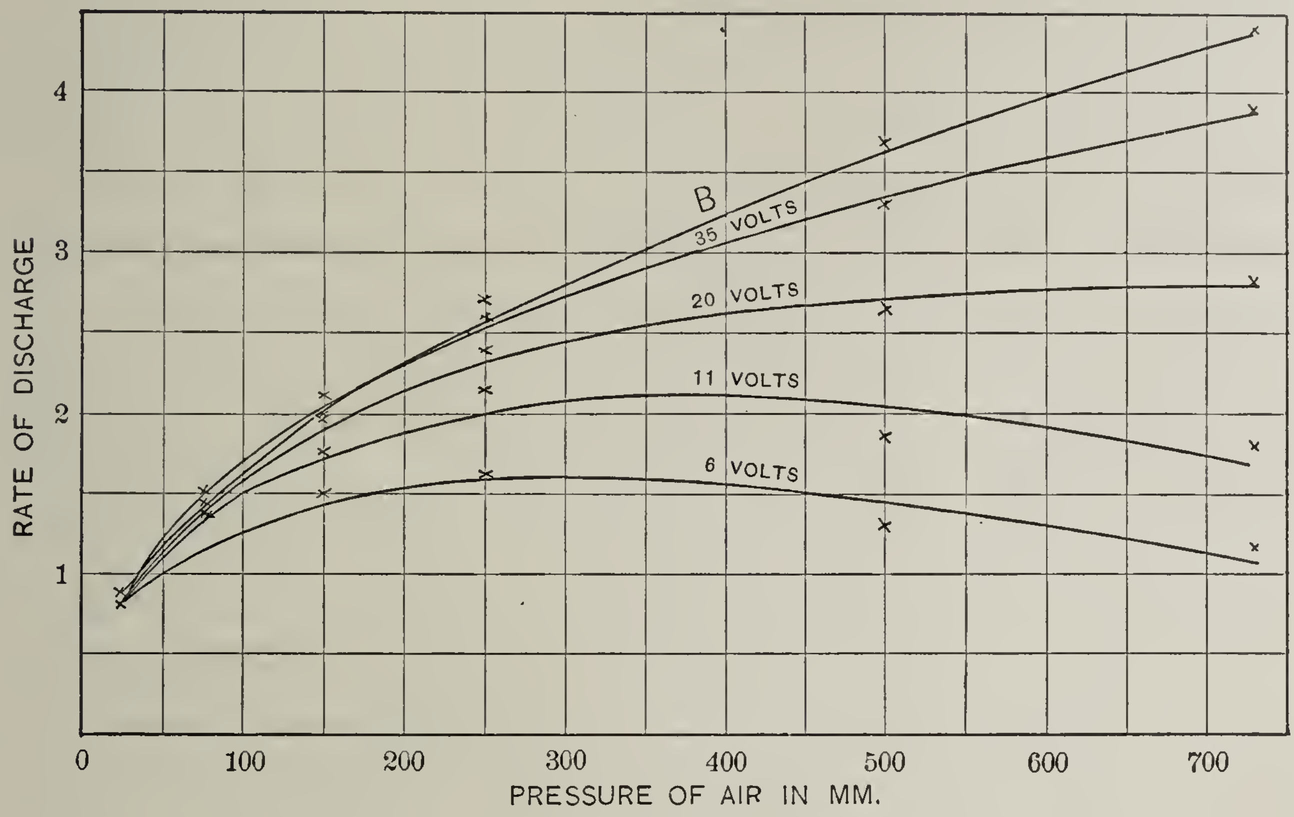

Fig. 2.

To find whether there was any leakage other than that to be studied, the rays were screened off from the plate by a thick piece of lead, and the tube was allowed to act for the same time as before. In this set of experiments such leakage was negligible.

Observations were also taken at a pressure of 12 mm. The values found were such as would be expected from the curves, but the rate of discharge was so small that the irregular leakage was appreciable as compared with that to be studied, and the data have therefore not been included in the table.

The difference in the essential character of the curves can be seen by comparing the curve for 35 volts with that for 6 volts. The former obeys fairly well the law given by Benoist and Hurmuzescu that the rate of discharge is proportional to the square root of the density. This agreement is shown by plotting from their data the curve marked B. The curves taken for higher potentials are practically the same as that given for 35 volts, but approach somewhat nearer to that given by Benoist and Hurmuzescu.

The curve for 6 volts obeys an entirely different law, and in fact it has a maximum at about 200 mm. The divergence from previous results in the case of this curve was so great that it seemed possible that there might be some error, at least as far as the maximum at 200 mm. was concerned.

Variation in method.—The results were therefore tested by a somewhat different method. The plate from which the discharge took place was connected to a given potential through a very high resistance. When the X-rays were allowed to strike the plate continuously, its potential would drop to a certain definite value. If V_1 was the potential of the point to which the high resistance was connected, V_2 that of the discharging plate and r the resistance, the current would be $\frac{V_1 - V_2}{r}$. V_1 and V_2 can be observed, and r is a constant which need not be known for comparative results. If one wishes to study the effect of other changes while V_2 is kept constant, V_1 may be varied so as to bring V_2 always to the same value. Two wires having cotton insulation were wound around each other, and there was sufficient leakage through the insulation to furnish the desired current. This resistance was measured by observing the rate at which it discharged a condenser of known capacity. It was found to be about 5,000 megohms.

In some respects this method would be preferable to the one used; but it is necessary that the tube should be in action several minutes before the discharging plate reaches a steady potential, and it was not found possible to keep the action of the tube constant for the time necessary to take several determinations. The data thus obtained were therefore less reliable than those which have been given. However, the method was of great value in verifying the conclusion reached by the first method. The curves found in the two cases were practically the same.

It is altogether possible that others have failed to find the results here described on account of using less intense rays. An aluminum window would be quite fatal, as far as getting any point of maximum discharge is concerned, unless a very efficient tube were used.

This method of performing the experiment has a bearing on the conclusion of Minchin, that there is no current in the case of discharge by X-rays which can be detected by means of a galvanometer.[1] Although no galvanometer was used in this experiment, it is difficult to see how there could be a fall of potential through a high resistance, nor why the fall of potential in this method should be proportional to the rate of discharge as found in the other method, if there is no current.

It is known that a metal tends to assume a final potential different from zero when X-rays strike upon it. It was thought that this effect might be complicating the phenomena being studied, but care was taken that the plate from which the discharge took place was surrounded by a grounded metal of the same kind. Under these circumstances the potential of the plate when it was disconnected from any source of electricity, and the rays were allowed to strike upon it continuously, differed but very little from zero, and any error due to this effect was entirely negligible.

Discharge with Different Potential Differences.—It is evident that another set of curves could be plotted, showing the relation, at different densities of the gas, between the rate of discharge and the potential. Thomson and Rutherford have studied the relation be-

Table II.

Initial Difference of Potential in Volts.		60	40	25	15	10	5	
75 mm.	Average Dif. of Pot.	56.6	37.3	22.4	12.6	7.9	3.6	
	Fall in Pot. in Volts.	3.9	3.4	3.65	3.65	3.4	2.05	
150 mm.	Average Dif. of Pot.	56.	36.5	21.7	12.1	7.5	3.7	
	Fall in Pot. in Volts.	5.	4.9	5.2	4.6	4.2	1.95	
400 mm.	Average Dif. of Pot.	55.6	36.	21.7	12.	8.	3.7	
	Fall in Pot. in Volts.	5.95	6.05	5.20	4.75	3.2	1.85	
760 mm.	Average Dif. of Pot.	54.2	35.	21.	12.5	8.1	3.9	
	Fall in Pot. in Volts.	8.6	8.1	6.45	3.9	2.85	1.55	
Initial Difference of Potential in Volts.		25	15	10	8	6	4	2
25 mm.	Average Dif. of Pot.	22.8	13.	8.1	6.1	4.4	2.7	1.2
	Fall in Pot. in Volts.	3.	2.8	2.9	3.	2.5	2.	1.2

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[1] Lond. Elec., 38, p. 789.

tween the potential and the current produced by the discharge at atmospheric pressure, and have shown that the current does not obey Ohm's law. As high potentials are reached, the current does not increase in a manner proportional to the increase in potential.

Table II. gives the data showing such relationship at different densities of the gas. It seemed best not to take the same data as given before, for the readings in that case did not extend over the range of potentials best suited for showing the effect we now desire to examine. Furthermore, in the previous set, the readings for the same density of gas but with different potential were separated in time, and consequently were taken under different conditions of the tube, so that they were not strictly comparable. In this set the readings were taken consecutively. There was some leakage in this case other than that due to the discharge being studied.

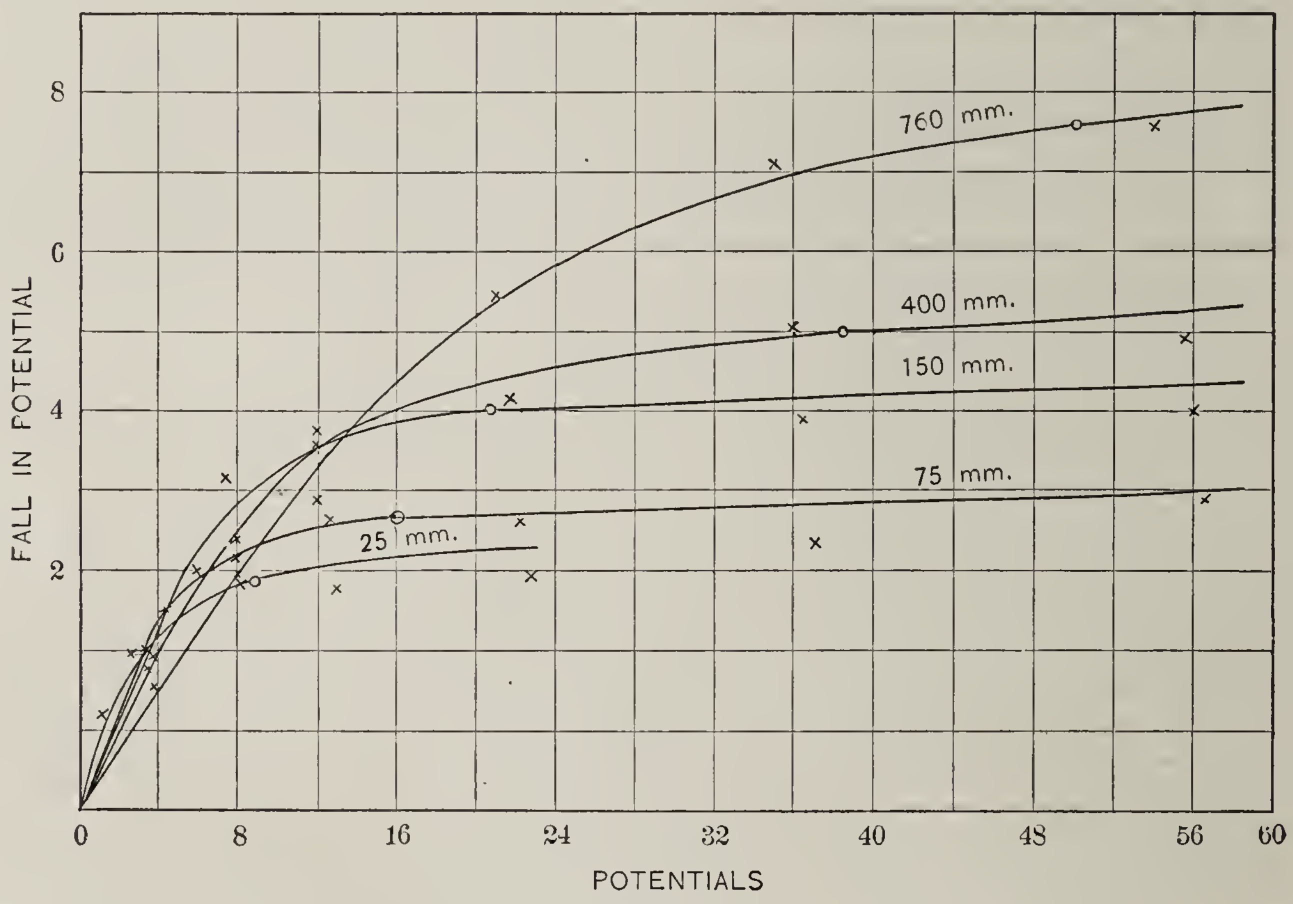

Fig. 3.

In Fig. 3, the corresponding curves are plotted. It will be seen that the limiting values of the current become smaller as the density of the gas becomes less. Points on the curves have been marked to indicate approximately the limiting currents.

These are what Thomson and Rutherford call the saturation points. If now the densities of the gas be plotted as abscissæ, and the currents at the saturation points as ordinates, a curve will be found quite similar in form with that given in Fig. 1 for 40 volts. This is, indeed, what we should expect; for it is evident from Fig. 2 that, after the saturation point is reached, the current is independent of the potential to which the body is charged, and we would be plotting saturation currents corresponding to certain densities. But in the case of a discharge with a potential of 40 volts, saturation currents were maintained all the time. So that these currents also depended only upon the densities of the air, and the two are in fact the same curve.

Discharge with Smaller Intensity of Rays.—The effect upon the form of the curve caused by varying the intensity of the rays was next studied. The curves which have already been plotted were taken with the tube near quite the plate to be discharged. A series was also taken with the tube at some distance from the plate, and with several inches of wood placed between the two.

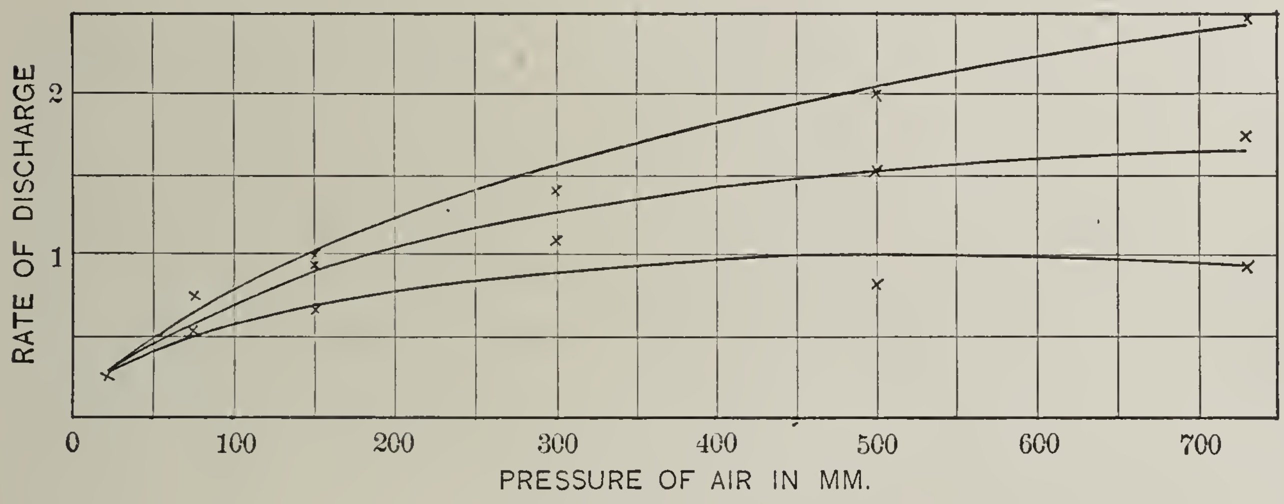

Fig. 4.

These curves are shown in Fig. 4. In none of them is there any maximum. It is thus clear that the form of the curve depends on the intensity of the rays used, and it therefore seemed unnecessary to try to find the form of the curves with any great degree of accuracy since there is no way of determining the intensity of the rays in any standard unit.

Density for Maximum Discharge.—I next attempted to find the relation between the density of the gas necessary for a maximum discharge, and the potential used. In the case of ultra-violet light, Stoletow[1] had found that if the distance between the plates is kept constant, this density varies directly as the difference of potential between the plates. I had found that when the intensity of the rays was small, the maximum point on the curve was not noticeable. The intensity of the rays was, therefore, increased, and several curves were plotted similar to those in Fig. 2, more attention being paid, however, to the variation in the maximum point. Such a set of curves is shown in Fig. 5.

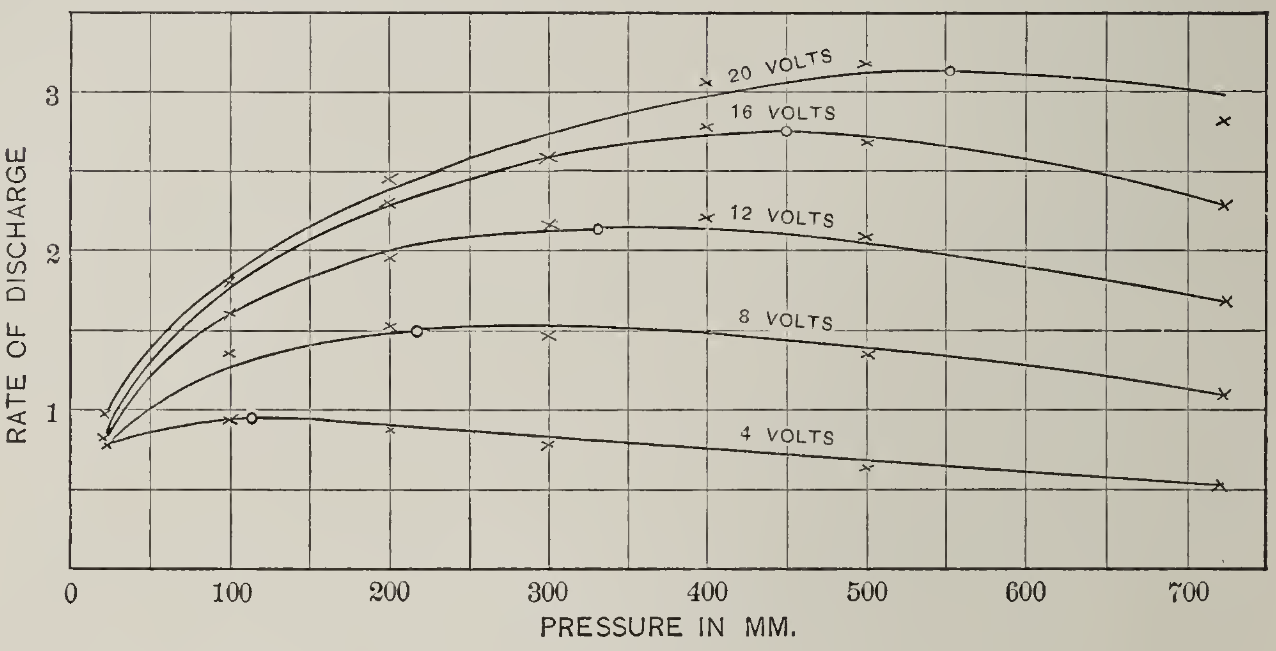

Fig. 5.

It was found that practically the same law applies in the case of X-rays, as holds when the discharge is produced by ultra-violet light; namely, that the density for a maximum discharge is proportional to the potential of the charged body. The points located by circles show where the maxima should occur in order to agree with this law.

However, it is noticeable that the conditions under which a maximum discharge occurs are quite different when it is produced by X-rays, and when produced by ultra-violet light. With X-rays the maximum was found when the pressure was 200 mm., the potential 8 volts, and the distance between the plate and the surrounding metal 2 cm.; while with ultra-violet light a maximum occurred with a pressure of 4.5 mm. when the potential was 100 volts, and the distance between the plates 8 mm.

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[1] Journal de Phys., 9, p. 471.

Density for Maximum Discharge and Intensity of Rays.—An attempt was made to find the manner in which the point of maximum discharge depended upon the intensity of the rays. The data proved to be of little value in this case. For when the intensity was somewhat diminished, the rate of discharge remained practically constant throughout a wide range of pressure, and it was difficult to tell whether an apparent maximum was produced by changing the pressure of the gas or by some irregularity in the action of the tube. The difficulty was made greater because it was necessary to use low potentials, and with the intensity small, the amount of discharge was small, and trouble was caused by ordinary leakage and slight fluctuations in the potential of the charged body. However, it was apparent that as the intensity became less, the pressure for a maximum discharge became greater, although this did not increase as fast as the intensity decreased.

Discharge through a Film of Paraffin.—Various modifications in the conditions of the experiments were tried. Perrin has stated that the discharge effect can be separated into a surface effect between the metal and gas, and a volume effect through the gas. I therefore thought that by covering the surface of the metal with a thin film of paraffin it might be possible to change the relation between the rate of discharge and the density of the air. Lord Kelvin, Beattie, and Smolan[1] state that there is no discharge through paraffin, but only a condenser effect. This point was first tested. The plate from which the discharge was to take place was connected through a high resistance to a source of electricity, as in the second method. The fall in potential of the charged plate when the X-rays acted on it continuously was nearly as great as it had been before the plate was covered with paraffin. It is difficult to see how this can be explained except on the hypothesis that a discharge actually takes place through the coating of the paraffin. The pressure of the air was then varied, and the curves were found to be the same as those in Fig. 2.

The plate to be discharged was then placed in the shadow of an opaque obstacle, and the rays allowed to strike only the air surrounding the plate. The form of the curve was not different from that previously obtained with rays causing the same rate of discharge. If, therefore, there is a surface effect it would appear to be small.

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[1] Nature, 55, 472 and 498.

Discharge with Normal Incidence.—Perrin has explained the difference between the results which he found when the density of the gas was varied, and those found by Benoist and Hurmuzescu, on the ground that the experiments were not performed under the same conditions. In his experiment, the incidence was grazing, while in those of Benoist and Hurmuzescu the incidence was normal. The apparatus was therefore changed so that the incidence was normal, in order to test this point. The following table shows the fall in potential for the different densities when the potential differences were 5 volts and 25 volts. This shows the same relation to exist when the incidence was normal as existed when it was grazing. The capacity in multiple with the plate was somewhat less in this case than it was when the first data were taken.

Initial Potential Difference in Volts.		5	25
25 mm	Fall in Potential in Volts.	1.2	1.3
75 mm		2.1	2.7
150 mm		2.4	3.5
300 mm		2.5	4.7
500 mm		2.3	5.7
760 mm		2.2	6.2

Physical Laboratory of
Cornell University.

(*To be Concluded.*)

THE SURFACE TENSION OF WASTE AND OF CERTAIN DILUTE AQUEOUS SOLUTIONS, DETERMINED BY THE METHOD OF RIPPLES. II.

(*Concluded.*)

By N. Ernest Dorsey.

Influence of Glass Plate.

Another point of prime importance is to have the glass plate, which generates the waves, well wetted by the liquid. If it is not, the crests will be so irregular and so distorted that no correct setting can be made. A microscope slide was first used but it was very difficult to get its surface in such a condition that water would wet it readily, and one lot of slides I was entirely unable to use; I tried to clean them with caustic potash, sulphuric acid, nitric acid, chromic acid ; I even boiled them in chromic acid, and still water would not readily wet them, and never under any condition did I succeed in wetting them uniformly. I finally used a piece of thin German plate mirror from which the silvering was removed. This was easily cleaned and there was no trouble in keeping it in such a condition that water and salt solutions would wet it readily and uniformly.

When I began this work I always took one reading within two centimeters of the fork, in order that I might run the fork with as small an amplitude as possible, so as to avoid reflected waves. The other reading was taken some five or ten waves further from the fork.

The results obtained by this method were very erratic. The wave-lengths found on any one day would agree fairly well among themselves, but those obtained on different days seldom agreed at all. Later on it was found that the apparent wave-length depended upon how many waves were included between the settings.

After trying nearly every plan that could be devised to find the trouble it was at last decided to measure each individual wave and see if these separate determinations would throw any light on the difficulty. In this series of measurements I went as close to the fork as I could measure, and found (although these separate determinations were very rough) that the apparent wave-length *increased* as the fork was approached. As a last resort I attributed this effect to the rise of the liquid against the plate which generates the ripples, although I did not think that this could effect waves two centimeters distant. To test this experimentally the plate that was well wetted was replaced by one coated with paraffin. It was now found that the waves apparently *decreased* as the fork was approached. This proved that, contrary to expectations, the effect of the meniscus makes itself felt two or three centimeters from the fork.

Plate Wetted.

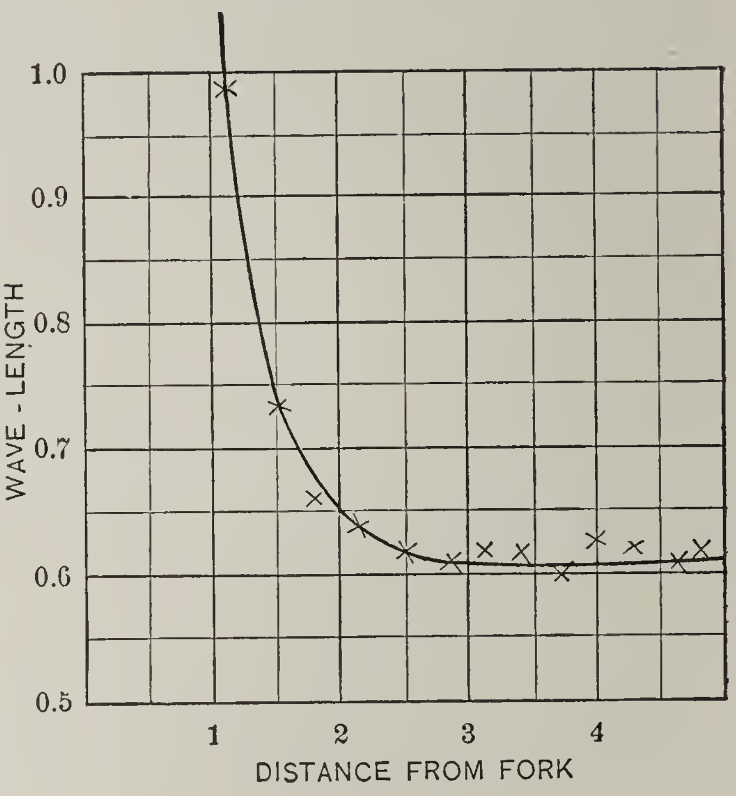

Fig. 3.

Two typical curves showing this effect are given in figures 3 and 4.

Plate not wetted.

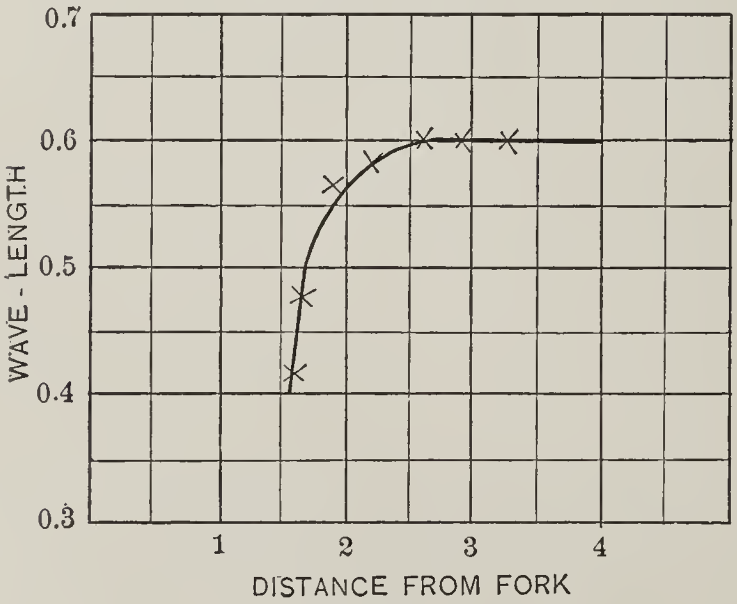

Fig. 4.

The magnitude of the error introduced in this way depends upon the amplitude of the waves, and this is probably the reason why the results obtained on different days were not comparable; for I frequently changed the amplitude of my fork so as to alter the distinctness of the field in the telescope. Many weeks were spent in locating this error; and after finding it no reading was taken nearer the fork than four centimeters. Under these conditions the measured wave-length does not depend upon the number of waves measured, nor upon the distance of the waves from the fork, nor upon the amplitude of the fork (so long as it is small); all these points were tested experimentally. I was very seldom troubled by reflected waves.

The mirrors M_1 and M_2 must be good plane metal mirrors. Owing to the double reflections, ordinary glass front mirrors can not be used, since in my arrangement the images overlap and distort one another. The silver surface obtained by depositing silver by Brashear's process[1] on good thin plate German mirror glass was used in this experiment.

PERIODS OF FORKS.

The fork F_2 is a large one with a mirror and a counter-weight screwed into the ends of its prongs, and belongs to a set of König forks used to show Lissajou's figures. The mirror was replaced by a second counter-weight, and the plate of glass which generated the ripples was fastened by screwing down the weight until the plate was clamped tightly between it and the end of the prong of the fork. This allowed the plate to be removed and cleaned without danger of wetting the fork. Sliding on the prongs were light brass weights that could be clamped wherever desired.

F_1 was also provided with sliding weights, and after putting on the strips of copper (S_2) I tuned it (by means of a vibration microscope) to unison with F_2 while the weights on F_2 were near the centre of its prongs, and while the plate of glass was dipping into water.

By means of the smoked glass and pendulum method the frequency of the vibration microscope, which was an octave above F_1, was determined. It made 125.8 complete vibrations per second. Then as a check the vibration microscope was again compared with F_1, and it was found that the ratio of their frequencies was still two to one.

The vibration microscope rather than F_1 was compared with the pendulum because it was more readily handled than the larger fork. This method, however, cannot be depended upon except to give the whole number of vibrations per second. To determine the exact frequency Michelson's method[1] was employed, but the results were carried to but two decimal places, as the frequency of a fork driven by a mercury contact does not seem to be more constant than this approximation. Now, in order to use this method, the clock must give exactly equal intervals of time ; Professor Michelson says that the intervals must differ by less than 0.002 second in rating a fork of 128 double vibrations per second. The clock at my disposal had a mercury contact, and I found that working alone I could not adjust this so accurately in the center of the arc of the pendulum as is required for consistent results. In any case, as Professor Michelson states, a mercury contact is not very good unless it is quite narrow in the direction of the swing of the pendulum.

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[1] Astrophysical Journal, 1, 252, 1895.

After trying many contacts and after working on the subject for some time, I finally devised one that is very simple and has so far given perfect satisfaction. A contact is desired that can be easily adjusted so as to give absolutely equal intervals of time ; so it seemed the best method would be to use the complete period of the pendulum instead of the half period, as is ordinarily adopted. This would obviate the difficulty of centering the apparatus.

The construction of the contact is as follows : A steel watch spring about 8 cm. long is soldered to a brass collar which is fastened by means of a screw to one end of a brass plate 4 cm. wide and 10 cm. long. The collar can be rotated about the axis of the screw, and clamped tightly at any angle desired. Perpendicular to the flat surface of the spring and about 5 cm. from the collar is soldered a platinum point which, when the spring is slightly depressed, dips into mercury in a cup attached to the brass plate but insulated from it. At the same place and perpendicular both to the platinum point and to the length of the spring is soldered the point of a fine steel sewing needle. This needle point must be quite fine and well polished. At the further end of the spring is fastened a mica vane which dips into oil contained in another cup fastened to the brass plate, and is intended to damp the vibrations of the spring. The brass plate to which these parts are attached is fastened to the back of the clock near the top of the pendulum and in such a position that the needle point comes near the centre of the arc of the pendulum.

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[1] Phil. Mag. (5), 15, 84–87, 1883.

To the pendulum rod is fastened a light brass collar on the back of which is soldered a short piece of steel watch spring which projects horizontally from the rod, and whose plane is inclined to the rod at an angle of about 50° or more. The lengths of the needle point and of this short piece of spring are such that when they are in place, and the pendulum is at rest they overlap by an amount just suffiicent to ensure the depressing of the point at each complete vibration of the pendulum. Both sides of the short spring are carefully polished, and the collar is so adjusted that the needle point comes just below the upper edge of the spring. The electric circuit is completed through the long spring and the mercury cup. Then when the pendulum swings in one direction the platinum point is depressed into the mercury and closes the circuit, and immediately rises and breaks it as the pendulum swings on; on the return swing the platinum point is slightly raised. If the damping vane works properly the vibrations of the spring will be practically dead beat, and we shall have contact made once only during each complete vibration of the pendulum.

Dr. J. F. Mohler kindly compared the clock I used with the astronomical one and found that each single swing of the pendulum corresponds to 1.000397 seconds; the mean solar day being the unit of time. Hence we are justified in taking the complete period of the pendulum as two seconds.

As any change in the positions of the blades S_2 affect the period of the fork, its frequency was often determined so as to avoid errors due to careless handling.

Amplitude of Waves.

We have seen that Lord Kelvin's equation applies to waves of infinitely small amplitude, so it is well to consider the amplitude of the waves used in this work. I have no way to measure its value directly, but a general idea of its smallness may be obtained by considering the fact that throughout the work the telescope was focused for a point one meter below the surface of the liquid. Since the beam of incident light is parallel, this means that in the neighborhood of a crest the radius of curvature is about two meters. This, together with the fact that these waves are only 0.48 cm. long, shows that their amplitudes must be extremely small. We can also see that the amplitudes are very small from the effect of the meniscus upon the apparent length of the waves at such a great distance as two centimeters from the plate (page 27).

Effect of Viscosity.

Before going further it is well to consider another point. How does the viscosity of the liquid affect the apparent wave-lengths? Professor Tait[1] has shown that viscosity does not affect the true wave-length, but simply introduces a damping factor of the form ε^{ax}, where a depends upon the wave-length and the viscosity of the liquid. Hence the apparent wave-length is the distance between the corresponding consecutive points where the curve $y = A\varepsilon^{-ax}\sin mx$ has a constant slope c. Hence it is necessary to find the roots of the equation.

$$\frac{dy}{dx} = -Aa\varepsilon^{-ax}\sin mx + Am\varepsilon^{-ax}\cos mx = c$$

or

$$B\cos(mx + \partial) = \frac{c}{A}\varepsilon^{+ax} \text{ where } B = \sqrt{a^2 + m^2},\ \tan\partial = \frac{a}{m}.$$

Since a is very small for water and dilute solutions, B is approximately equal to m. The maximum value of c is at the point $x = 0$, where $c = Am$. Hence the roots of the equation are the abscissæ of the points of intersection of the curves

$$z = \cos(mx + \partial) \text{ and } z = E\varepsilon^{ax} = E(1 + ax)$$

approximately; $E \ngtr 1$. By setting near a crest E can be made very small and hence the slope of the line $z = E\varepsilon^{ax}$ will also be small, practically zero, and the apparent wave-length must be very nearly $\frac{2\pi}{m}$, which is the true wave-length. If we could make $c = 0$; the apparent and real wave-lengths would be exactly equal.

To test experimentally the error that might be introduced if c were not zero, the telescope was turned so as to look on one side of the crests at points having as great a slope as could be observed, and the apparent wave-length was measured. The telescope was then turned so as to look at corresponding points on the other side of the crests and the wave-length was again determined. Each time I measured over a distance equal to 20 waves, the interval usually employed in the rest of the work. The maximum difference observed amounted to one-fifth of one per cent.

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[1] Proc. Roy. Soc. Edinb., 17, 110–115, 1890.

The apparent field swept over by the spider line in turning the telescope from one of these positions to the other is over two centimeters ; and I can easily adjust the line to the centre of this field to within one millimeter, *i. e.*, one-twentieth of the total angle through which I turned the telescope. If the error was proportional to the angle, this would correspond to 0.01 per cent; ; but the error varies almost as the cosine of the angle. Hence the error introduced is negligible.

Effect of Particles.

In this same article Professor Tait shows that the effect of a "uniform film of oxide or dust, in *separate* particles which adhere to and move with the surface" is to increase the wave-length if there is no viscosity. If terms involving the first power of the viscosity must be taken into account, the surface layer will not affect the wave-length, but will aid viscosity in causing the waves to subside as they advance.

Water and Salts.

The water used in these experiments was especially distilled by Dr. W. T. Mather, and was the kind he used for his electrolytic work. He distilled it from chromic acid, and from potassium permanganate, and condensed it in a block tin condenser. It was collected in a bottle used for no other purpose, and which was kept always covered or stoppered. It was transferred from this to a larger bottle that had a hole bored near its bottom so that the water could be drawn off through a piece of black rubber tubing and a glass nozzle. This tube and nozzle always remained filled with water, which protected their inner surfaces from impurities in the air. The glass nozzle was removed and cleaned before drawing the water. In this way it was hoped to get water as pure as possible. I might mention here that the water and solutions used in this work were not free from air; but Röntgen and Schneider[1] have shown that the effect of dissolved air is entirely negligible at ordinary pressures.

I used Eimer and Amend's chemically pure salts.

The temperatures of the water and solutions were determined by means of a Bender and Hobein thermometer of Normalglas graduated to tenths of a degree centigrade.

The densities of the solutions of sodium chloride, of sodium carbonate, and of zinc sulphate were calculated from the values given by Kohlrausch and Hallwachs[2]; those for potassium chloride from the results obtained by Bender[3]; and those for potassium carbonate from Gerlach's values as given in Landolt and Börnstein's "Physikalisch-Chemische Tabellen." The densities for water were taken from Marek's values.[4]

METHOD OF OBSERVATION.

Before each series of readings the tray, the glass plate, the brass hoop for cleaning the surface of the liquid, the thermometer, and the stirring rod were washed, first with tap water, then with an alcoholic solution of caustic potash, then with running tap water, then with chromic acid, and finally they were well rinsed in running tap water. Then everything was placed in position in the iron box, water was run into the tray until the lower edge of the glass plate dipped just under the surface. I then expanded the hoop, put the lid on the box, and brought the iron slab to rest as soon as possible. Then the forks were set in vibration, the spider line of the telescope was brought into coincidence with a crest near the middle of the pan, and the amplitude of the vibrations was increased until the crest was almost out of focus. As stated before, the waves appear stationary. Then the shunt circuit by which F_2 was run was broken and the fork was allowed to come to rest at its free period. The fork F_1 being still vibrating, the crests will have an apparent motion unless the free period of F_2 coincides with the period of F_1. If the two did not coincide the weights on F_2 were moved until they did. By care the depth of the water can be adjusted so that this correction is seldom necessary; but unless the two forks are in unison the crests are almost sure to oscillate, on account of the imperfection of the mercury contact. The plate which generates the waves should never leave the liquid, but it should dip below the surface as little as possible. If it dips deeply into the liquid the waves always appear distorted.

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[1] Wied. Ann., 17, 207, 1882.

[2] Wied. Ann., 53, 14, 1894.

[3] Wied. Ann., 22, 179, 1884.

[4] Wied. Ann., 44, 171, 1891.

Having brought the forks to unison the spider line is brought successively into coincidence with three consecutive crests near scale division 17. Then the carriage is screwed along and readings are taken on the fortieth, forty-first and forty-second crest beyond the one on which the first setting was made. The thermometer is then read, the hoop is lifted from the water, is contracted, replaced into the water and expanded, and another series of six readings is taken. Then a known weight of salt is added, and the solution is stirred until the salt is dissolved. Then readings are taken as for water; then more salt is added, and so on until readings have been taken on water and on solutions of (as a rule) four different concentrations. Then the volume of the solution is measured. I measure the volume last so that the purity of the water may not be affected by any trace of grease that might be on the measuring vessel.

Since each crest is seen in two positions, measuring over forty crests gives the length of twenty waves. Such a series of readings as I have just described occupies about two hours; and to make sure that the surface in this length of time does not become contaminated so as to affect my results, a check series was taken on water. The first readings gave $T_{180.9}=73.10$ or $T_{180}=73.24$; the last, about two and one-half hours later, gave $T_{190.3}=72.91$, or $T_{180}=73.18$. Hence the results obtained from such a series of readings that extend over about two hours should be not appreciably affected by the contamination of the surface by the air.

By this plan of work we obtain for each concentration two sets of three independent determinations of the wave-length. If the average of these two sets agree fairly well, the surface tension is calculated from their mean. If they do not agree well another series is taken and the surface tension is calculated from the weighted mean of the three series.

TEMPERATURE REDUCTION.

To reduce the results to the standard temperature, eighteen degrees centigrade, I used the formula $T_t = T_0\ (1 - at)$, and took $a = 0.0020$ which is the average of the following values :

Brunner	0.0019	Pogg. Ann., 70, 481.
Braun	0.0020	Winklemann, Handb. d. Phys., I., p. 467.
Frankenheim	0.0018	Pogg. Ann., 72, 177, 222, 1847.
Wolf	0.0018	Ann. de chim. et de phys. (3), 49, 269, 1857.
Volkmann	0.0018	Wied. Ann., 17, 353, 1882.
Timberg	0.0022	Wied. Ann., 30, 545, 1887.
Jäger	0.0023	Acad. de Wiss. in Wien., 100, 245, 1891.
Cantor	0.0021	Wied. Ann., 47, 399, 1892.
Humphreys and Mohler	0.0020	Phys. Review, 2, 387, 1895.
Sentis	0.0020	J. de Phys. (3), 6, 183, 1897.
	0.0020	

In this formula t is the temperature in degrees centigrade ; T_t is the surface tension at t°C. ; and T_0 is the surface tension at 0°C.

Since the salt solutions used were very dilute I used the same temperature coefficient (0.0020) for them ; Sentis says that in any case the temperature coefficient for aqueous salt solutions differs from that for water by an amount less than his experimental error, which is about the same as mine.

RESULTS.

The results obtained are given in the tables and are represented graphically in Fig. 5.

In the tables the concentration of the solution is given in gram molecules per liter, and is placed in the column marked $\frac{\text{gm. mol.}}{\text{liter}}$; t denotes the temperature in degrees centigrade ; n is the frequency of the fork ; ρ is the density ; T_t is the surface tension at t°C., and T_{18} is the surface tension at 18°C. ; both T_t and T_{18} are expressed in dynes per centimeter ; λ_1 is the observed wave-length in terms of scale divisions of the engine, one division $= 1.0328$ cm.

TABLE I.

NaCl

Gm. mol. / liter.	t	n	λ_1	$\frac{Tt}{\rho}$	Tt	T_{18}
.0	16.0	63.07	0.4858	73.72	73.64	73.34
0.067	17.2	"	0.4852	73.44	73.56	73.44
0.14	17.8	"	0.4849	73.29	73.58	73.55
0.22	18.2	"	0.4843	73.01	73.57	73.60
0.29	18.6	"	0.4843	73.01	73.78	73.87

NaCl.

.0	16.3	63.07	0.4858	73.72	73.64	73.38
0.12	16.7	"	0.4852	73.44	73.70	73.50
0.29	17.4	"	0.4849	73.29	74.08	73.99
0.46	17.7	"	0.4840	72.88	74.16	74.12
0.63	18.5	"	0.4837	72.73	74.37	74.45

NaCl.

.0	13.1	63.07	0.4865	74.03	73.99	73.25
0.17	13.5	"	0.4859	73.76	74.23	73.55
0.33	14.1	"	0.4855	73.57	74.52	73.92
0.51	15.0	"	0.4854	73.52	75.00	74.54
0.68	19.0	"	0.4841	72.92	74.83	74.99

NaCl.

.0	16.4	62.96	0.4863	73.67	73.59	73.35
0.088	17.0	"	0.4860	73.54	73.72	73.57
0.17	17.3	"	0.4855	73.30	73.73	73.62
0.25	17.4	"	0.4849	73.01	73.68	73.59

NaCl.

.0	18.6	62.96	0.4854	73.25	73.14	73.23
0.37	19.3	"	0.4839	72.56	73.55	73.75
0.72	19.8	"	0.4825	71.91	73.91	74.17
1.11	20.1	"	0.4815	71.45	74.53	74.86
1.46	20.0	"	0.4805	70.99	75.02	75.33

TABLE I.—*Continued.*

$\frac{\text{Gm. mol.}}{\text{liter.}}$	t	n	λ_1	$\frac{Tt}{\rho}$	Tt	T_{18}
			NaCl.			
.0	19.3	62.96	0.4842	72.69	72.57	72.77
0.397	20.3	“	0.4837	72.47	73 53	73.88
0.468	20.9	“	0.4837	72.47	73.72	74.17
0.549	21.3	“	0.4833	72.28	73.76	74.27
0.62	21.4	“	0.4824	71.86	73.54	74.06
0.703	21.3	“	0.4817	71.55	73.46	73.97
0.768	21.2	“	0.4821	71.73	73.83	74.32
			NaCl.			
.0	15.9	62.96	0.4860	73.54	73.47	73.15
0.34	16.2	“	0.4852	73.16	74.11	73.84
0.40	16.5	“	0.4850	73.06	74.19	73.96
0.46	16.8	“	0.4845	72.76	74.06	73.88
0.52	16.9	“	0.4847	72.92	74.40	74.23
0.58	17.1	“	0.4841	72.65	74.30	74.16
0.64	17.2	“	0.4840	72.56	74.38	74.26
0.76	17.3	“	0.4840	72.56	74.72	74.61
0.88	17.4	“	0.4831	72.19	74.69	74.60
			KCl.			
.0	18.6	62.87	0.4854	73.02	72.91	73.00
0.09	18.6	“	0.4861	73.35	73.49	73.58
0.19	18.4	“	0.4858	73.21	73.76	73.82
0.27	18.3	“	0.4852	72.93	73.75	73.80
0.35	18.2	“	0.4853	72.98	74 05	74.08
0.58	18.1	“	0.4855	73.07	74.52	74.54
			KCl.			
.0	18.7	62.78	0.4855	72.84	72.73	72.84
0.338	19.3	“	0.4851	72.66	73.68	73.88
0.557	19.9	“	0.4849	72.57	73.96	74.25
0.771	20.2	“	0.4839	72.11	74.55	74.89
0.986	20.3	“	0.4827	71.56	74.62	74.98
1.204	20.3	“	0.4823	71.37	75.11	75.48

Table I.—*Continued.*

KCl.

$\frac{\text{Gm. mol.}}{\text{liter.}}$	t	n	λ_1	$\frac{T_t}{\rho}$	T_t	T_{18}
.0	18.0	62.87	0.4863	73.43	73 33	73.33
0.19	17.3	"	0.4851	72.88	73.44	73.33
0.37	16.8	"	0.4847	72.69	73.86	73.68
0.55	16.4	"	0.4847	72.69	74.46	74.21
0.73	16.0	"	0.4844	72.55	75.00	74.69
0.92	15.8	"	0.4836	72.19	74.96	74.62

KCl.

.0	21.1	62.78	0.4860	73.08	72.81	73.28
0.221	20.6	"	0.4848	72.52	73.14	73.54
0.446	20.3	"	0.4843	72.29	73.64	73.99

½(K_2CO_3 + 2 aq)

.0	18.3	62.86	0.4862	73.38	73.28	73.33
0.22	19.2	"	0.4850	72.82	73.85	74.04
0.44	19.9	"	0.4833	72.04	73.97	74.27
0.66	20.4	"	0.4813	71.13	73.90	74.27
0.88	20.8	"	0.4802	70.61	74.41	74.84

½(K_2CO_3 + 2 aq)

.0	13.7	62.86	0.4864	73.46	73.41	72.76
0.096	13.7	"	0.4860	73.28	73.81	73.16
0.294	13.6	"	0.4851	72.86	74.29	73.62
0.512	13.5	"	0.4836	72.17	74 43	73.74
0.713	13.5	"	0.4826	71.70	74.95	74.26
0.915	13.5	"	0.4812	71.07	75.14	74.45

½(Na_2CO_3 + 10 aq).

.0	18.5	62.85	0.4857	73.11	73.00	73.08
0.061	18.5	"	0.4855	73.02	73.20	73.28
0.187	18.1	"	0.4853	72.92	73.61	73.63
0.310	17.6	"	0.4849	72.74	73.95	73.89
0.432	17.4	"	0.4840	72.33	74.04	73.95

Table I.—*Continued.*

$\frac{1}{2}(Na_2CO_3 + 10\ aq)$.

Gm. mol. / liter.	t	n	λ_1	$\frac{Tt}{\rho}$	Tt	T_{18}
.0	17.9	62.86	0.4857	73.14	73.04	73.03
0.115	17.6	"	0.4855	73.05	73.42	73.36
0.228	17.1	"	0.4848	72.73	73.54	73.40
0.344	16.6	"	0.4848	72.73	74.02	73.81
0.458	16.3	"	0.4838	72.27	73.99	73.73
0.574	15.9	"	0.4837	72.22	74.40	74.08

$\frac{1}{2}(ZnSO_4 + 7\ aq)$.

.0	18.1	62.78	0.4867	73.40	73.30	73.31
0.129	18.7	"	0.4850	72.65	73.32	73.43
0.257	18.7	"	0.4840	72.16	73.58	73.69
0.39	18.7	"	0.4822	71.47	73.64	73.75

$\frac{1}{2}(ZnSO_4 + 7\ aq)$.

.0	17.7	62.78	0.4864	73.26	73.16	73.12
0.283	18.2	"	0.4832	71.79	73.36	73.39
0.567	18.4	"	0.4804	70.51	73.65	73.71

$\frac{1}{2}(ZnSO_4 + 7\ aq)$.

.0	18.6	62.78	0.4867	73.40	73.12	73.21
0.115	19.6	"	0.4853	72.76	73.33	73.58
0.173	19.7	"	0.4851	72.66	73.48	73.74
0.216	19.7	"	0.4845	72.38	73.54	73.80
0.263	19.7	"	0.4832	71.79	73.22	73.48

$\frac{1}{2}(ZnSO_4 + 7\ aq)$.

.0	18.4	62.78	0.4866	73.35	73.24	73.31
0.0435	19.5	"	0.4857	72.94	73.07	73.30
0.0887	20.2	"	0.4847	72.47	72.88	73.38
0.133	20.7	"	0.4850	72.61	73.28	73.69
0.35	20.5	"	0.4832	71.79	73.71	74.09

Table I.—*Concluded.*

$\frac{1}{2}(ZnSO_4 + 7\,aq)$.

$\frac{\text{Gm. mol.}}{\text{liter.}}$	t	n	λ_1	$\frac{Tt}{\rho}$	Tt	T_{18}
.0	18.3	62.78	0.4868	73.45	73.34	73.39
0.0199	18.4	"	0.4860	73.08	73.11	73.17
0.066	18.8	"	0.4856	72.89	73.18	73.30
0.108	18.9	"	0.4853	72.76	73.30	73.44
0.152	18.7	"	0.4850	72.61	73.42	73.52
0.286	18.7	"	0.4841	72.21	73.80	73.90
0.330	18.5	"	0.4830	71.71	73.54	73.62
0.372	18.4	"	0.4829	71.65	73.75	73.79
0.414	18.3	"	0.4827	71.55	73.87	73.91

$\frac{1}{2}(ZnSO_4)$.

$\frac{\text{Gm. mol.}}{\text{liter.}}$	t	n	λ_1	$\frac{Tt}{\rho}$	Tt	T_{18}
.0	14.8	62.85	0.4870	73.74	73.68	73.20
0.18	15.2	"	0.4848	72.70	73.72	73.30
0.37	15.5	"	0.4839	72.31	74.43	74.05
0.555	15.9	"	0.4822	71.53	74.67	74.35
0.736	16.2	"	0.4804	70.71	74.82	74.54
0.919	16.4	"	0.4789	70.03	75.06	74 81

In the first series for potassium chloride the two forks were not in unison when the surface tension of water was determined, but were afterwards adjusted. This accounts for the low value found for water. In the case of the carbonates, for which but two series of determinations were made, the two sets of points are denoted on the plate by crosses and by crosses in circles respectively. It will be noticed that the two series for potassium carbonate do not coincide but lie parallel to one another. If there were any impurity in the body of the water that decreased its surface tension, it could not be removed by the flexible hoop, and probably would lower the whole curve as we observe in this case. Those points for zinc sulphate which are represented by crosses in circles were obtained from a series of readings on solutions of the anhydrous sulphate. This contained quite a little of the sulphide, which floated around in the solution but which apparently produced no effect on the wave-lengths of the ripples. The values obtained by Sentis are represented by circles.

It is evident that these results can be best represented by straight lines. If we write

$$T_s = T_w + Kc$$

where T_s is the surface tension of the solution containing c gram-equivalents per liter, and T_w is the surface-tension of water at the same temperature, and K is a constant, we find K from the plotted results. The values found for K by various observers are given in the following table: those assigned to Sentis were calculated from his observations on solutions of 0.56 and 1.13 normal concentrations.

Table II.

			H_2O			
t	n	λ_1	$\frac{Tt}{\rho}$	Tt	T_{18}	T_0
16.0	63.07	0.4858	73.72	73.64	73.34	
16.3	63.07	0.4858	73.72	73.64	73.38	
13.1	63.07	0.4865	74.03	73.99	73.25	
16.4	62.96	0.4863	73.64	73.59	73.35	
18.6	62.96	0.4854	73.25	73.14	73.23	
19.3	62.96	0.4842	72.69	72.57	72.77	
15.9	62.96	0.4860	73.54	73.47	73.15	
18.6	62.87	0.4854	73.02	72.91	73.00	
18.7	62.78	0.4855	72.84	72.73	72.84	
21.1	62.78	0.4860	73.08	72.81	73.28	
18.0	62.87	0.4863	73.43	73.33	73.33	
18.3	62.86	0.4862	73.38	73.28	73.33	
13.7	62.86	0.4864	73.46	73.41	72.76	
17.9	62.86	0.4857	73.14	73.04	73.03	
18.5	62.85	0.4857	73.11	73.00	73.08	
18.1	62.78	0.4867	73.40	73.30	73.31	
17.7	62.78	0.4864	73.26	73.16	73.12	
18.6	62.78	0.4867	73.40	73.12	73.21	
18.4	62.78	0.4866	73.35	73.24	73.31	
18.3	62.78	0.4868	73.45	73.34	73.39	
14.8	62.85	0.4870	73.74	73.68	73.20	
17.44				73.25	73.17	
Omitting $T_{18} = 72.77$; 72.84; 72.76 we find the following values:						
17.48				73.32	73.24	75.98

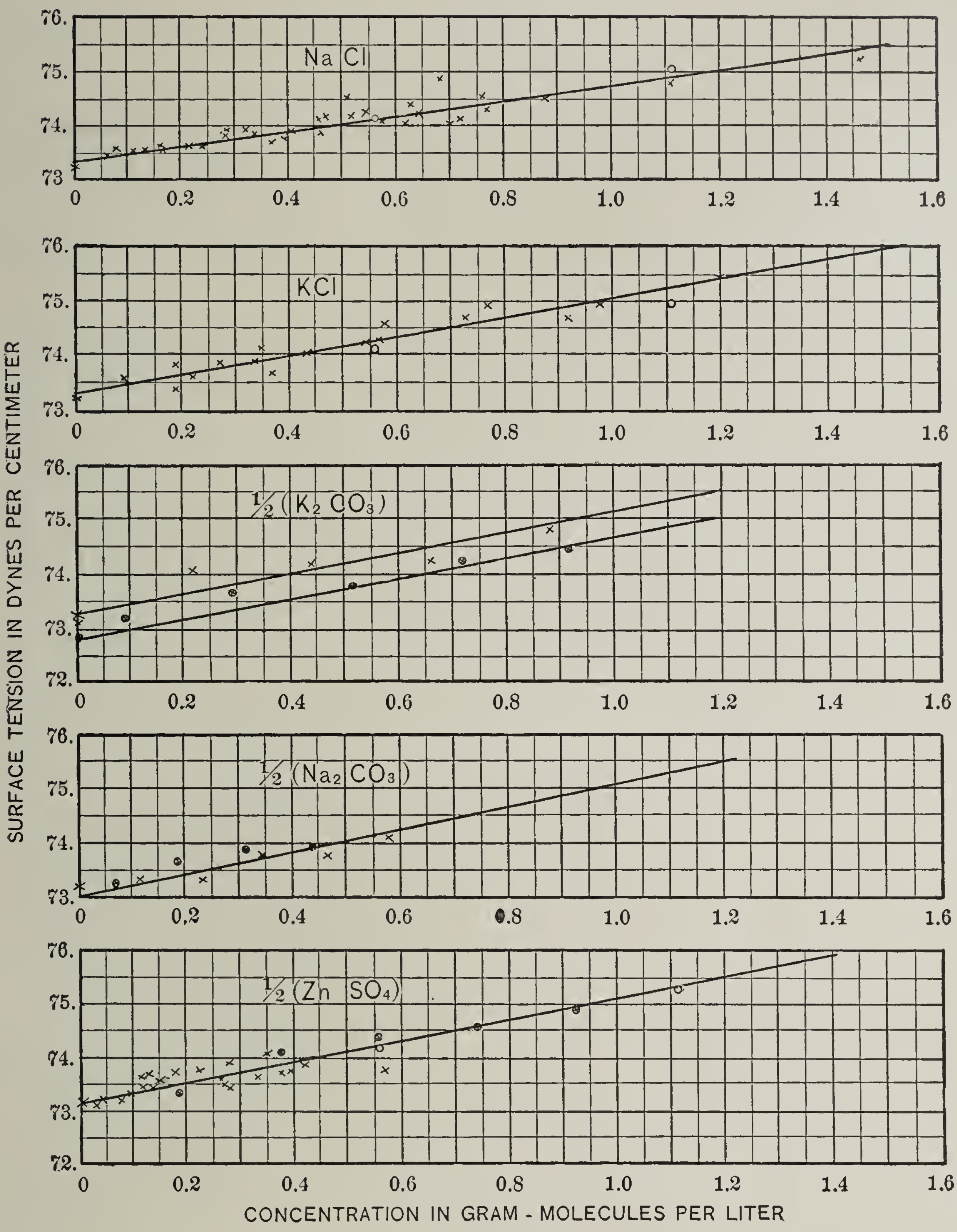

Fig. 5.

	Volkmann	Quincke	Sentis	Rother	Dorsey
NaCl ; K=	1.59	1.57	1.57	1.38	1.53
KCl ; K=	1.41	1.57	1.41	1.47	1.71
½(Na_2CO_3) ; K=	0.987	1.57	—	—	2.00
½(K_2CO_3) ; K =	1.78	1.57	—	—	1.77
½($ZnSO_4$) ; K=	—	—	1.78	—	1.86

All of Volkmann's curves appear to become steeper as they approach zero concentration, and his curves for K_2CO_3 and Na_2CO_3

cross near the origin so that the curve for Na_2CO_3 for dilute solutions become steeper than the one for K_2CO_3.

The comparison of the results of this work with those obtained by other observers must not be pushed too far, because they have worked exclusively on solutions of greater concentration than half normal, while most of my observations are on solutions less concentrated than that.

The average of all observations taken on water gives $T_{17^\circ.44}=73.25$, or $T_{18^\circ}=73.17$ which is rather low. If we omit the values $T_{18^\circ}=72.77$, 72.84 and 72.76, which are certainly too low, we find the averages given in the last line of the table, viz., $T_{17^\circ.48}=73.32$ which gives $T_{18^\circ}=73.24$ and $T_0=75.98$. This is lower than the values found by Quincke, but agrees very well with the results obtained by Volkmann, Hall, Sentis, Rayleigh and others.

In conclusion I wish to thank Professor Rowland and Dr. Ames for their suggestions and encouragement throughout the entire course of the work.

Johns Hopkins University, May, 1897.

ON THE USE OF THE INTERFEROMETER IN THE STUDY OF ELECTRIC WAVES.

By G. F. Hull.

Outline.

1. Introduction.—Use of the interferometer in the study of light.
2. Historical statement.—Analogy between light and electric radiation not complete. Work of Sarasin and de la Rive, . . . for Hertzian apparatus proving that the wave-length measured depends on the receiver. Is the radiation from a Hertzian vibrator simple or complex? Experiments and theory dealing with this point. Bearing of this theory in our experiments.
3. Apparatus.—Interferometer; conditions governing choice of vibrator and receiver; sensitiveness; absence of diffracted and scattered radiation; preliminary observations.
4. Measurement of λ.—Degree of accuracy.
5. Estimation of " δ' " from the interference curve.—Proof that this does not give the logarithmic decrement of the vibrator, as has been supposed. Comparison of experiment with theory.
6. Influence of receiver upon the interference curve.—Inference as to the period and damping coefficient of our receiver.
7. Dependence of λ upon vibrator.—No agreement between values of λ found by other observers for vibrators similar to our own.
8. Determination of μ.—Degree of accuracy. Conclusion.

1. Use of the Interferometer in the Study of Light.

The interferometer, as a special form of refractometer has been called by Professor Michelson, has been found to be a very powerful instrument in the study of light. By its means the nature and wave-length of the radiation from a source and the index of refraction of a transparent medium have been found with the greatest accuracy. It is true that a grating or prism spectroscope must be used for the study of complex light, but for comparatively simple radiation the interferometer possesses a far greater power of analysis. It was with the object of constructing an interferometer for electric waves and of using it in their study that this research was undertaken.

2. Here it may be well to point out that electrical and light oscillations differ in an important respect. The breadth of a spectral line of a homogeneous gas has been accounted for upon various assumptions among which is the one that the oscillations of the molecules of the gas gradually die down owing to its communicating energy to the surrounding medium, or to other causes. Experiments, however, have failed to verify this assumption.[1] In the case of electrical radiation, on the other hand, theory indicates and experiment proves that the oscillations are always damped.

Another difference is in the apparatus used for detecting the radiation in the two cases. For light, the eye is the usual detector. Though it is sensitive for radiation lying within a very small range, it has no period of its own and does not possess the power of influencing the measured wave-length. That electrical receivers, as a rule, have this power has been proved by the experiments of Sarasin and de la Rive,[2] Klemencic and Czarmak,[3] and quite recently by those of Wiedeburg.[4] In fact, for the different forms of Hertzian vibrators and receivers used by these physicists, the wave-length measured depended almost entirely upon the receiver.

The explanation of this fact led to the important question whether the radiation from a Hertzian vibrator is simple or complex. Upon this point opinions have differed. From the phenomenon of multiple-resonance as discovered by Sarasin and de la Rive[2] and Klemencic and Czarmak[3] many physicists were led to the conclusion that the radiation is complex. The experiment of Zehnder[5] showing that the rays of electric force are analyzed by a grating into a spectrum pointed in the same direction, while Garbasso and Aschkinass[6] found that rays, in passing through a prism made up of glass plates upon which were pasted strips of tinfoil, are dispersed and concluded that rays of electric force may be considered not necessarily as monochromatic but, with the same justification as in the case of light, as composite. On the other hand Bjerknes[1] using Lecher's arrangement[2] attempted to prove that the radiation is due to a simple damped oscillation of the form $Ae^{-\gamma^t} \sin (at + a')$. The theory of his method, which differed from the later work of Klemencic and Czarmak chiefly in this that he used waves along wires, while they used waves in air is as follows: Assuming that the oscillations are of the form $Ae^{-at} \sin at$, he finds the effect upon the receiver of two infinite trains of waves, one of which is direct from the vibrator and the other reflected from the ends of the wires. This effect (using notation and limits of integration to suit our experiment) is given by

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[1] A. A. Michelson, Astrophysical Journal, p. 251, November, 1895.

[2] Sarasin and de la Rive, Compt. Rend., 115, p. 439, 1892.

[3] Klemencic and Czarmak, Wied. Ann., Vol. 50, 1893.

[4] Wiedeburg, Wied. Ann., Vol. 59, p. 496, 1896.

[5] Zehnder, Wied. Ann., Vol. 53, p. 172, 1894.

[6] Garbasso and Aschkinass, Wied. Ann., Vol. 53, p. 534, 1894.

$$J = \int_0^{\frac{2x}{v}} Ae^{-2at} \sin^2 at\, dt + \int_0^{\infty} dt \left[Ae^{-at} \sin at + Ae^{-a\left(t + \frac{2x}{v}\right)} \sin a \left(t + \frac{2x}{v} \right) \right]^2$$

$$= \frac{A^2}{2a} \left(1 + e^{\frac{2ax}{v}} \cos 2a \frac{x}{v} \right).$$

In Bjerknes' experiment x was the distance of the receiver from the end of the wires; in our experiments it is the distance of a mirror from the "zero position"; in both cases $2x$ is the total difference in path of the two trains of waves. Thus the curve whose ordinates are proportional to the intensity of the electrical radiation for a point whose abscissa is x, is leaving out a constant, a damped cosine curve. It was found that the experimental agreed fairly well with the theoretical curve. It was therefore concluded that the oscillations followed the law assumed.

This conclusion is open to two possible criticisms which are suggested by the questions: first, may not the wires with their terminal plates or the receiver exercise a selective action on the radiation, and second, may not the interference curve be obtained assuming another law for the oscillations. Indeed Bjerknes found that the interference curve was influenced by the change of the distances between the plates of the vibrator and the terminal plates of the wires—that as this distance increased the waves were not so rapidly damped but were fainter—a result which agrees with the theory that the more feeble the radiation the less rapid is its decay. Probably the wires were too long to affect the result but that is a point which in general should be considered.

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[1] Bjerknes, Wied. Ann., Vol. 44, p. 513, 1891.

[2] Lecher, Wied. Ann., Vol. 41, p. 850, 1890.

The results obtained by Sarasin and de la Rive and the other physicists named above, indicating that the radiation is complex, may be accounted for by assuming that the oscillations are simple and damped and that the receiver has a period of its own and is comparatively undamped. On the other hand, neglecting the criticisms brought forward, it would be difficult to account for the regularity of the interference curve obtained by Bjerknes on the assumption that the radiation is complex. On the whole the question has not been satisfactorily settled. We are justified in expecting that the problem may be approximately solved by means of the interferometer.

The question which concerns us is this—even if electric radiation is, in general, sufficiently simple to be analyzed by the interferometer, how shall we interpret the interference curve—is its form influenced by the receiver? The latter point is a matter for experiment to decide in the case of the receiver used.

Note.—The theory which applies here is rather unsatisfactory. Assuming that the receiver, when no outside forces act, executes damped pendulum motions and that the oscillations of the vibrator are of the same nature we obtain the equation :

$$\frac{d^2\varphi}{dt^2}+\beta\frac{d\varphi}{dt}+(\beta^2+b^2)\varphi=Ae^{-\alpha t}\sin(at+e)$$

where φ represents the potential difference between the parts of the receiver. Let us impose the conditions $\varphi=\frac{d\varphi}{dt}=0$ when $t=0$. The solution may be expressed in the form—

$$\varphi=C\sin\left(\frac{a+b}{2}t+c'\right) \text{ where}$$

$$C=\frac{A^2}{(a^2-b^2)^2+(a^2-b^2)(a^2-\beta^2)}e^{-(\alpha+\beta)t}\left[e^{-(\alpha-\beta)t}+e^{(\alpha-\beta)t}-2\cos(a-b)t+\right.$$

$$2\frac{1+\cos\varepsilon}{a+b}\left((a-b)e^{\alpha-\beta}-(a-b)\cos(a-b)t-(\alpha-\beta)\sin(a-b)t\right)+$$

$$2\frac{\sin\ \varepsilon}{a+b}\left((\alpha-\beta)e^{(\alpha-\beta)t}-(\alpha-\beta)\cos(a-b)t-(a-b)\sin(a-b)t\right)\Big].$$

The action on the receiver due to the two trains of waves is

$$\int_0^{\frac{2x}{v}}\left(\varphi(t)\right)^2 dt+\int_0^{\infty}\left[\varphi(t)+\varphi\left(t+\frac{2x}{v}\right)\right]^2 dt=2K+2\int_0^{\infty}\varphi(t)\varphi\left(t+\frac{2x}{v}\right)dt.$$

This is the equation of the interference curve. But in view of the uncertainty of the hypothesis and the complexity of the result, it seems needless to expect assistance from this theory.

The work so far described was performed with the Hertzian vibrator and receivers. I am not aware that any corresponding work has been done with spheres unless it is that of Lang[1] and of Bose.[2] Lang obtained interference effects by a method analogous to that of Quinke in sound. But it seems probable that reflections from the tubes to which the radiation was led would obscure the effect. Bose states that the radiation from a sphere under the conditions existing in his experiment gives a *line* spectrum from which the inference is to be drawn that the vibrations are not only simple but very slightly damped. He also states that the wave-lengths measured were independent of the periodicity of the receiver. If this were true it must have been on account of the dead beat character of the receiver—not on account of the method used.

The experiments to be described in this paper will show (1) that the oscillation due to electrical disturbances on spherical conductors are for the most part, rather highly damped sine functions, (2) that the interference curve is influenced by the receiver—sometimes to such an extent as to change the apparent wave-length.

3. APPARATUS.

The interference device shown in Fig. 1 was patterned after Michelson's simple but effective interferometer. Radiation coming from the parabolic mirror P falls at an incidence of 45° on a *separating surface* S which reflects part to M and transmits the rest which goes to *M'*. Of the radiation reflected from *M* and *M'* the parts transmitted and reflected by *S* fall together upon *R* (See Fig. 1). Difference of path is obtained by the motion of one or both mirrors. To obtain as large an effect as possible in *R* the separating surface should transmit and reflect one-half of the radiation falling on it. For this purpose there were pasted on a piece of cardboard, half a metre square, strips of tin foil 1x40 cms. At first the adjacent edges of two neighboring strips were one centimeter apart. Later it was found necessary to remove every alternate strip, making the adjacent edges 3 cms. apart.[1] The reflection and transmis-

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[1] Victor von Lang. Phil. Mag., Feb. 1896.

[2] Bose. Proc. Roy. Soc., Oct. 16, 1896.

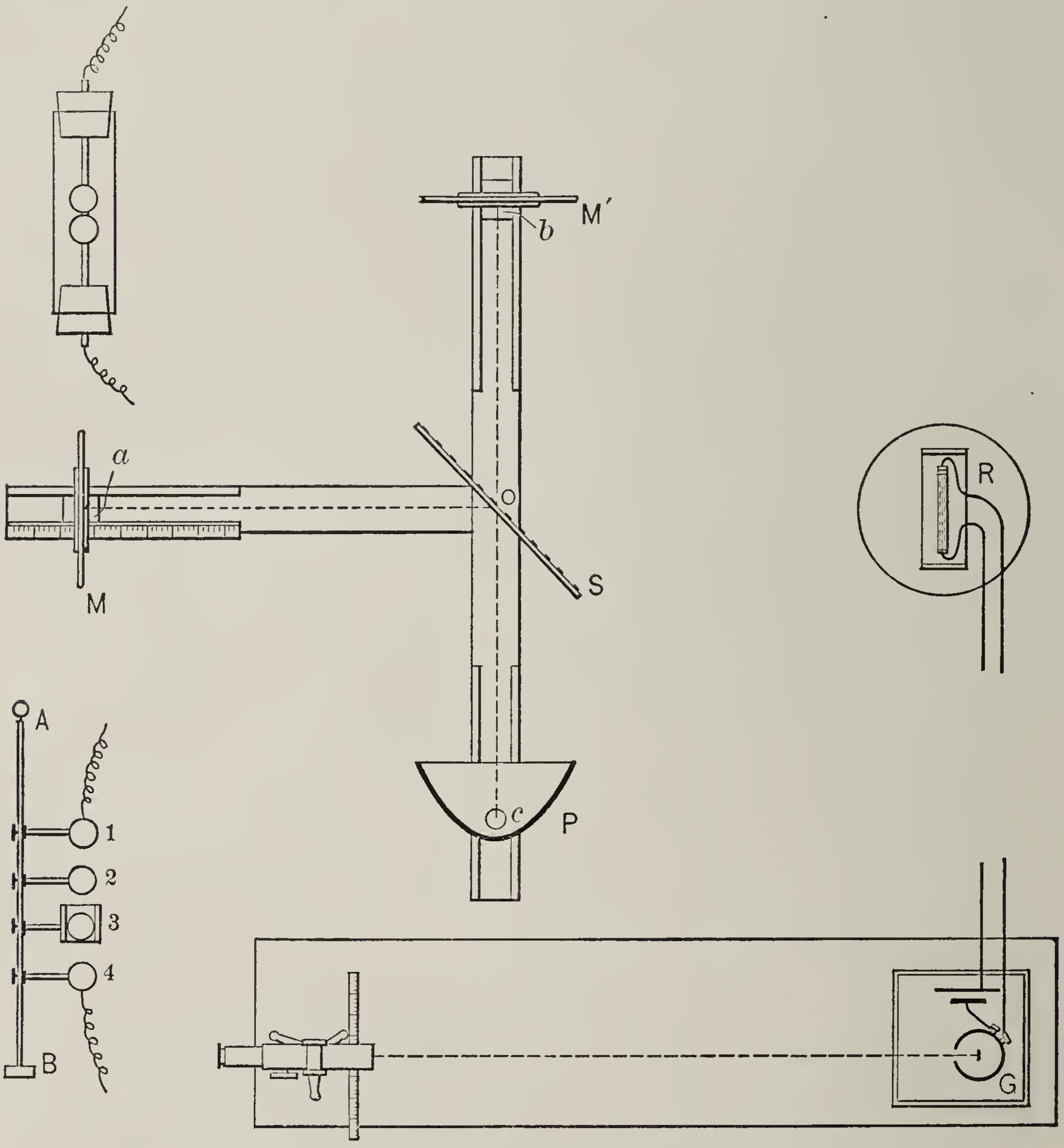

Fig. 1.

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[1] This agrees with the conclusion of Lord Layleigh deduced from theory, viz.—a narrow aperture parallel to the electric vibrations transmits very much less than is reflected by a conductor elongated in the same direction. Phil. Mag., April, 1897, p. 272.

sion powers of the grating were determined by well-known means and it was found that when the strips were parallel to the electric oscillations, this grating reflected and transmitted about half of the radiation falling on it.

The wooden arms *oa* and *bc* were grooved so that the supports carrying the mirrors *M*, *M'* and *P* were movable along *oa*, *ob*, *oc*. The angle *aob* was approximately a right angle and the mirrors were placed each normal to its arm by optical means. The mirrors *M* and *M'* were of plate glass with the silvered surface towards O. The parabolic zinc mirror *P* had a focal length of 2.5 cm., a height of 40 cms. and an aperture of 30x40 cm. As it was intended to use waves not longer than 10 cm. the dimensions of *M* and *M'* and of the aperture of *P* were large enough to obviate any serious diffraction effects. The focal length of *P* was chosen about one-fourth of the wave-length in order that the direct and reflected waves might be in the same phase. But as the radiation was too strong (unless the galvanometer was short circuited) this precaution was unnecessary.

The arrangement for the vibrator usually consisted of two spheres connected by fine wires to the secondary terminals of the induction coil and sparking to each other in oil. Each sphere was screwed on the end of a hard rubber rod which passed through a rubber cork and this was inserted in a glass tube about 10 cm. long and 2.5 to 3 cm. in diameter. This tube was partially filled with oil—usually vaseline oil, sometimes paraffine oil. A small hole blown in the side of the tube allowed one, by rubbing paper between the spheres, to partially clean them without removing the vibrator from its position. Righi's arrangement[1] was also used. Four hard rubber rods each carrying a sphere, were attached to a vertical rod in the mirror *P*. All distances were adjustable. The third sphere was fastened by means of melted wax in a small glass cylinder of such a size that it held sufficient oil for the spark gap and yet allowed the spheres 2 and 3 to approach until their surfaces touched. The advantage of this arrangement was that all the spark gaps could be fairly well cleaned by paper without removing the spheres. The rod *AB* could be rotated so that the spheres could be brought out towards the aperture of *P* or moved back to the focal line.

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[1] Righi, Mem. del R. Acad. dei Sc. del Inst. di Bologna, T. IV., 1894.

The receiver chosen for this experiment must fulfill certain conditions. It must be sufficiently sensitive to respond to radiation reaching it after reflection and diffraction, sufficiently constant to give quantitative results, and as we desire to determine the nature of the radiation it should respond equally well to waves of all periods sent out by the vibrator. Now it has been shown that Hertzian receivers do not fulfill this last condition. It was thought that a receiver which responds to electric waves of periods extending over a wide range would measure accurately the radiation falling on it. Having had some experience in the use of the *coherer* (as named by Lodge, though the name seems misleading) we thought that if it could be made constant it would answer our purpose. Consequently we used a Branley or Lodge receiver consisting of small nails or pieces of wire about 1 cm. long, in a glass tube 15 or 20 cm. long and filled with a lubricating oil. This was placed in the focal line of a semi-circular zinc reflector of aperature 10x20 cm. A sheet of tin foil over part of the aperture served to shut out, if necessary, part of the radiation. Lead covered wires connected the receiver with the galvanometer and battery which were enclosed in a tin box. Thus, with the exception of the aperture of the mirror, the whole circuit was enclosed in metal.

In order to make the deflections small enough to be read, the galvanometer, a simple D'Arsonval, was short circuited by a resistance varying from one-fourth to one-half of an ohm. It is hardly necessary to state that in finding the proper grating the ordinary phenomena of polarized radiation were observed. For example, if the receiver, grating and vibrator were placed in a line, with the grating immediately in front of the receiver and its strips horizontal (perpendicular to the vibrator), almost all the radiation passed through. If the strips were vertical only one-half passed through. A block of wood with its fibres vertical transmitted much less than it did when its fibres were horizontal.

It was found that the receiver responded nearly as well when the electric oscillations were vertical as when they were horizontal. In this respect, therefore, it differs from the Hertzian receiver. In order that the diffracted and scattered radiation be as small as possible, the axis of the mirror, and therefore, also the vibrator, were vertical, for in this position it was not necessary to have metallic ends in the mirror. In our experiments the total scattered radiation was only three per cent. of the effect when there was zero difference of path in the two beams. Considering the fact that the receiver was sensitive to radiation reaching it in all directions, whether from the air or from the wires leading to the galvanometer, and that it responded slightly to the long waves of the induction coil, which was not enclosed in metal, this result was felt to be satisfactory. A small induction coil with an ordinary automatic interruptor, operated by one storage cell, completed the apparatus. A number of interruptors were tried, but they were not found to be more constant than the one belonging to the coil, and were more complicated. Usually the key was closed for about one second. Closing it for a longer time did not increase the deflection, but only served to destroy the sparking surfaces.

4. Measurement of λ. Fig. 2, Curve 1.

This figure was found by taking the mean of four deflections for every position of the mirror which was moved always in one direction and through 5 mm. each time. After 80 or 100 readings the vibrator became nearly useless, showing a deterioration with time. So in all observations after this the mirror was moved forward and back: *e. g.*, to determine the relative readings for three positions, *a*, *b*, *c*, the series *a b c b a* was taken. If we are concerned with the wave-length and not with the interference curve it is necessary to find only the maxima and minima positions. But the accuracy with which one of these positions can be determined depends on the *sharpness* of the curve at that point. Consequently the first step in the determination of λ (*or even of* μ) is to roughly plot the interference curve, observe the sharpness of the maxima and minima and choose the sharper of these from which is to be estimated the desired quantity.

In order to see to what degree of accuracy λ may be found, the positions of four successive minima were observed. The following results were obtained, the numbers referring to the position of the mirror on the scale

	Second behind.	First behind.	First before.	Second before.	Mean for λ/2.
Expt. 1. . .	6.45	11.	15.55	20.2	4.58 cm.
Expt. 2. . .	6.55	10.95	15.5	20.1	4.53 cm.
Expt. 3. . .	6.5	10.97	15.53	20.17	4.55 cm.

and the mean of these gives $\lambda/2=4.56$ cm. to within a fraction of one per cent. The vibrator consisted of two spheres 1.93 cm. in diameter arranged as in Fig. 1.

5. ESTIMATION OF "δ'."

If we accept the theory given on page 5 as correct we can from the curve find the logarithmic decrement of the vibrator. Let a_1, a_2, a_3, be readings for $x=0=\lambda/4$ and $\lambda/2$ respectively, then

$$aT=\delta=2\log\frac{a_1-a_2}{a_3-a_2}$$

Now the first minimum and maximum of the curve (1) correspond nearly with the values of $x=\lambda/4$ and $\lambda/2$ respectively. Using these values in the curve (1) we get, since $a_1=14.5$, $a_2=3.3$, $a_3=9.1$,

$$\delta=0.74.$$

But it is certain that this theory, though it may apply when receivers are used which are dead beat compared with the vibrators, does not apply in the case of Klemencic and Czarmak's experiments nor in our own; for we have found that the interference curve is influenced by the receiver—a fact which this theory neglects. However, it is interesting to compare the values of $2\log\frac{a_1-a_2}{a_3-a_2}$ (which we will call δ' and which has some connection with the damping of the vibrator or receiver or both), found by Klemencic and Czarmak and others, for Hertzian vibrators and receivers, with the corresponding value for the spheres and coherer. Their values varied from 0.3 to 0.5 while ours varied from 0.5 to 1. This indicates either a more complex radiation or a higher damping coefficient in the case of our apparatus than in that of the earlier experiment. But these values (0.5 to 1) are considerably lower than that suggested by theory. For the amplitude of the oscillations produced by the disturbance of a distribution of electricity on a sphere is where a=radius of the sphere, $=e^{-\frac{v}{2a}t} \quad =e^{-\frac{\delta}{T}t} \quad \therefore \quad \delta=\frac{v}{2a}T=\lambda/2a.$ In the case of this vibrator $a=1$ nearly and $\lambda=9.+$, hence $\delta=\underline{4.5.}$ This, however, is the case of an isolated sphere, a condition which does not exist in our experiment. But in later work we used one sphere and a small knob sparking to it and the value of did not exceed the value 1.0. Hence it appears either that δ' is not equal to δ or that Thomson's theory does not agree with experiment.[1]

6. Influence of Receiver upon the Interference Curve.

So far nothing has been said regarding the wave-length measured as to whether it is that of the vibrator or is due to a property of the receiver. It seemed that the period of the latter might depend: on (1) the dimensions and arrangement of the nails; (2) the dimensions of the tube. In order to vary these quantities three other receivers were used. In receiver II there were nails like those in receiver I, but the tube and terminals were different. Receiver III had in place of nails, copper wire cut up into lengths varying from 2 mm. to 2 cm. In receiver IV were used steel spheres about 4 mm. in diameter. Two vibrators were used, No. 1 already referred to and No. 2, which differed from it only in having spheres .93 cm. in diameter. The interference curves are given in Fig. 2. If the receiver exercised no influence on the interference curve the curves 1, 3, 5, 7, should be similar, as also should 2, 4, 6, 8. We reach, therefore, a very definite conclusion, the interference curve depends partly on the receiver.

In the curves 1, 3, 5, 7, we notice that λ has approximately the value 9.2 cm. This value then we take to be the wave-length of the radiation from vibrator I.

The curve (No. 2) for vibrator 2 and receiver 1 was observed many times and it always assumed forms between the dotted and continuous lines of No. 2. It seems due to two components, one of wave-length 8.6 cm. and the other of half that wave-length. In

[1] J. J. Thompson, "Recent Researches," p. 370.

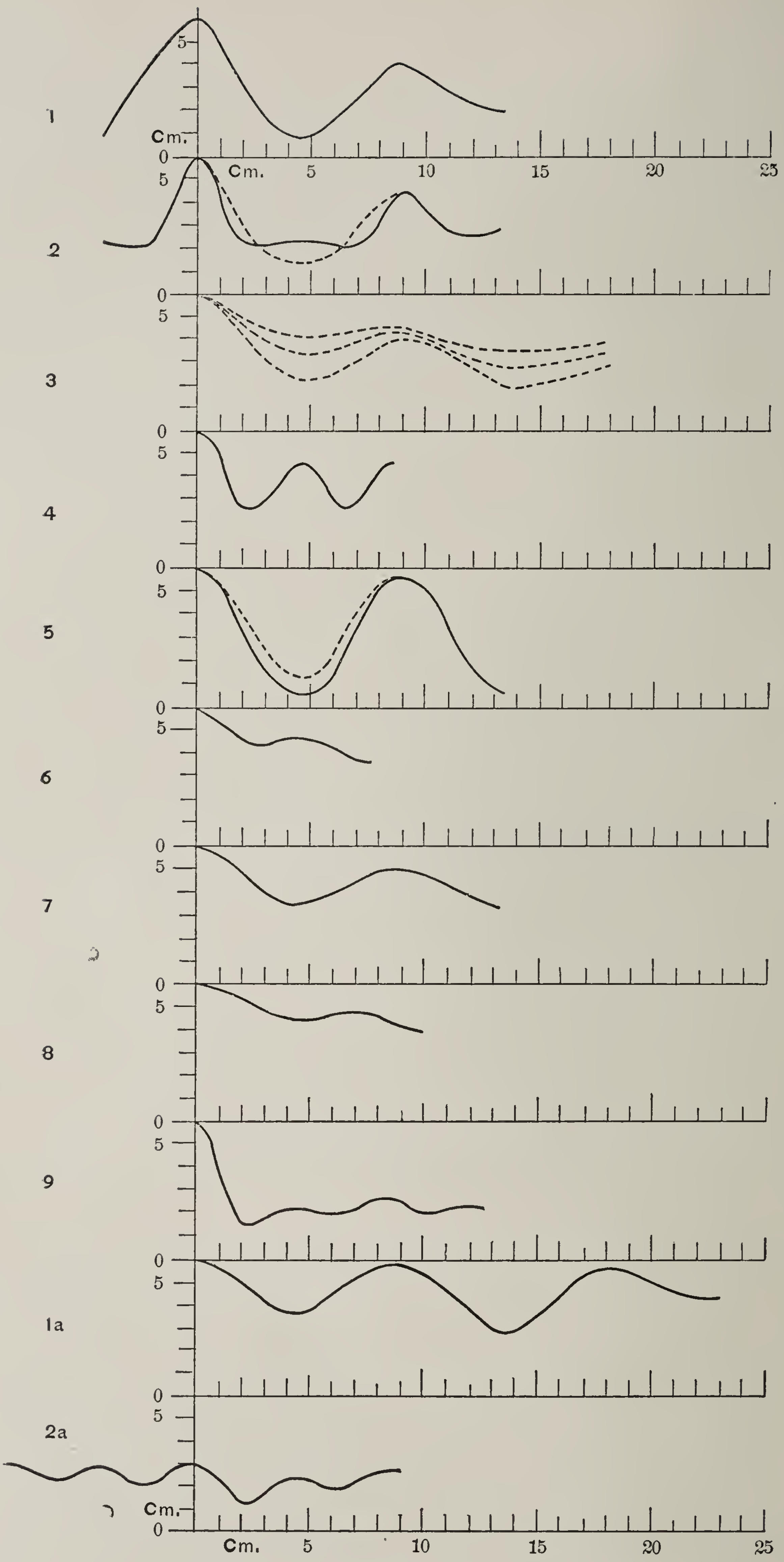

Fig. 2.

view of the fact that receivers II and III for this vibrator gave a wave-length of 4.2 to 4.4 cm., we are led to believe that the component of the shorter wave-length is due to the vibrator and that the other component, which is present probably to a slight extent in the radiator, owes its prominence to a resonance action of the receiver. The interpretation that receiver I has a period corresponding to the long wave receives confirmation in the fact that the curve for vibrator I has such well defined maxima and minima. But, whatever interpretation be placed on the interference curve, it is evident that it cannot always be taken to accurately represent the radiation. On the other hand the wave-length for either vibrator is maintained by all the receivers (leaving out curevs 6, 7, 8, for which the wave-lengths are very indefinite). This fact shows that our receiver has far less influence on the interference curve than has the Hertzian receiver. In other words, the damping coefficient of the former is far less greater than that of the latter. Our expectation, therefore, that the coherer used is highly damped has been fulfilled, but it appears from our work that its damping coefficient is of the same order of magnitude as that of the vibrator and consequently it influences the interference curve.

In view of this action of the receiver we are not able to accurately analyze the radiation, but using a number of receivers we are able to arrive at a fair estimate of its nature. We are thus led to state that the chief component of the radiation from spheres is due to a damped oscillation.

7. DEPENDENCE OF λ UPON VIBRATOR.

Besides the vibrators 1 and 2 we used the following arrangements: Vibrator No. 3, consisting of one sphere of No. 1, to which sparked a small platinum bead. The interference curve is given in No. 1_a. The wave-length is the same as that of No. 1, but the maxima and minima are less pronounced. Vibrator 4 differed from No. 3 only in having one of the spheres of vibrator 2 in place of one from No. 1. The interference curve is that of 2_a. Here again the wave-length is the same as when the two spheres are used. We also used two steel spheres 0.79 cm. in diameter together and singly. Their curves, which are similar to those already found, are not given. The wave-length was about 4 c.m. Righi's form of vibrator, with the two spheres of No. 1 sparking in oil and those of No. 2 sparking in air, was used. The wave-length was again approximately equal to that of No. 1.

It is here seen that the wave-length is nearly proportional to the diameter of the sphere (the law is expressed more nearly by $\lambda = m(r+a)$ where m and a are constants and $r=$ radius of the sphere), and that it is independent of the arrangement by means of which the discharge is brought about. I was not able to observe any change of wave-length due to a change of sparkling distance. It is interesting to compare the values of λ obtained by other observers with those which we have found. It will be seen that no agreement exists between the results of any two observers.

Dia. of Spheres.	Value of λ.	Arrangem't of Vibrator.	Receiver.	Method of Meas'g λ.	Observer.	Reference.
mm.	mm.		Nail			
39.7	88	Righi's	Coherer.	Interference	Lang.	
24.4	82	"	"	through	"	Wied. Ann., 3,
10.6	80	"	"	two tubes.	"	1896.
80.	200	Righi's	Silver on	Interference	Righi.	L'eclairage Elec-
37.5	106	"	Glass. Spark	"	"	trique. No. 3,
8.	26	"	Observed.	"	"	p. 360. 1895.
7.8	18.4	Lodge's one	Spiral spring	Grating.	Bose.	Proc. Roy. Soc.,
		Sphere.	Coherer.			LX, 361, 1896.
19.3	91	Righi's and	Coherers.	Interferom-		See pages 13–16.
9.3	43	Lodge's.		eter.		
7.9	40					
Length of						Physical Review.
cylinder.			Thermo-			1896.
8x1 mm.	50	Righi's	element.	?	Cole.	Wied. Ann.
1.3x0.5 "	6	"	"	Interference	Lebedew.	No. 9, 1895.

Surely but one interpretation can be placed upon the values of λ as found by Lang. It is that the wave-length measured is due chiefly to the receiver.

8. DETERMINATION OF μ.

In order to determine accurately the index of refraction of a medium transparent to electric waves by the interferometer it is necessary that we have an interference curve with well defined maxima and minima. The influence of the receiver on this curve is, of course, a matter of no concern except that it should assist in giving this definition. Neither does the question of the nature of the radiation concern us unless (what is exceedingly improbable) the medium in question exercises selective absorption for these long waves.

To prevent the radiation which is reflected from the surfaces of the plate from reaching the receiver, the plate should be inclined at an angle to the rays. If the index of refraction is not much greater than unity, this precaution is unnecessary. If, however, the plate is kept normal to the rays, its thickness should be varied gradually through at least a wave-length and the mean of all the results taken.[1]

In order to find the degree of accuracy with which the value of μ may be determined by this method, observations were made with a block of wood 8.2 cm. thick. The displacements of the first and second minima on both sides of the zero position, and with the block before either mirror were found. Using the formula $\mu=(d+\delta)/d$ the following six values were obtained: 1.87, 1.80, 1.86, 1.83, 1.88, 1.83; giving a mean of 1.845 with a mean square error of 0.027. In these observations the fibres of the wood were horizontal. When the fibres were vertical the values 2.03, 2.01 were found. These results are sufficient to show (1) that the error in determining μ need not exceed one per cent., (2) that the wood used is doubly refracting.

Conclusion.—The points of difference between light and electric radiation which have been noticed in §2 tend to make the problem of the analysis of the radiation more difficult in the case of electricity than in that of light. For there are added to the variables of the vibrator those of the receiver. However, when we find that there is present in all the interference curves for one vibrator a certain

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[1] The only theory which appears to us to apply is complicated and will not assist us. The action on the receiver is given by

$$\int_0^{\frac{2x}{v}} [\phi(t)]^2\,dt + \int_0^{\infty} [\phi(t)+F(t)]^2 dt \text{ where } F(t) \text{ is found}$$

by taking account of the radiation reflected successively from the surfaces of the plate. This itself is a complicated expression.

element we are safe in saying that that element is due to an oscillation of a definite period on the vibrator. We are not justified at present in going further than this.

The following are the conclusions at which we arrive:

1. The interference curves depend on both the vibrator and receiver.

2. The influence of each of these varies.

3. The logarithmic decrement of the receiver is of the same order of magnitude as that of the vibrator.

4. The chief component of the radiation and the period of the vibrator may be determined from a number of interference curves.

5. This receiver could be used to analyze the radiation where the oscillations are but slightly damped.

6. The error in determining the index of refraction need not exceed one per cent.

7. Progress is to be looked for in obtaining: (*a*) a more constant source; (*b*) a more constant, dead beat, yet sensitive receiver.

This research was carried on at the Ryerson Physical Laboratory under the direction of Head Professor A. A. Michelson, to whom as also to associate Professor Stratton, I am indebted for many suggestions and much encouragement.

RYERSON PHYSICAL LABORATORY,
UNIVERSITY OF CHICAGO, May 1, 1897.

MINOR CONTRIBUTION.

On a Possible Change of Weight in Chemical Reactions.

By Fernando Sanford and Lillian E. Ray.

THE investigation here described was suggested by the notable paper on the same subject published by Landolt in 1893.[1] In this paper, Landolt refers to the fact that while the preponderance of experimental evidence is opposed to any change in the weight of a substance due to a change in its chemical structure, still different investigations have led to conflicting results. For the purpose of making a conclusive test with a variety of reactions, Landolt, in the years 1890–92, made a very careful and exact series of weighings, both before and after their chemical reactions with each other, of substances sealed in glass tubes.

The reactions investigated by Landolt were as follows:

1. Reaction between silver sulphate and ferrous sulphate, producing silver and ferric sulphate.
2. Reduction of iodine from iodic acid and hydriodic acid.
3. Reaction of iodine and sodium sulphite, producing hydriodic acid and sodium sulphate.
4. Reaction between chloral hydrate and potassium hydroxide, producing chloroform and potassium formate.
5. Chloral hydrate and water, before and after solution.

The table on next page shows the results of Landolt's experiments.[2]

These results, obtained as they were with the utmost possible care and by the use of the best apparatus obtainable, leave the question as to the possible change of weight in chemical reactions still open. In the three silver reactions, the apparent loss of weight is, on an average, more than nine times as great as the probable error of weighing, and is, as will be seen by referring to the original paper, from two to five times as great as the maximum variation between any two weighings of a series. The six reactions between iodic acid and hydriodic acid likewise show in every case a loss of weight, which, while less regular than in the silver reactions, still averages nine times as great as the probable error of weighing. In the other reactions, the observed change was sometimes positive and sometimes negative, and in some cases less than the probable error of weighing.

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[1] Zeitschrift f. physik. Chem. XII., 1, 1894.

[2] *Ibid*, XII., 31, 1893.

	I.	*II.*	*III.*	*IV.*	*V.*
Reaction.	Experiment.	Weight of reacting substances.	Observed change in weighing.	Probable error of weighing.	Change in weight for 100 g. of reagents.
Silver Sulphate and ferrous sulphate.	Ia.	114.2 g.	—0.167 mg.	±0.014 mg.	—0.146 mg.
	Ib.	"	—0.131 "	" 0.020 "	—0.115 "
	II.	171.3 "	—0.130 "	" 0.012 "	—0.076 "
Iodic acid and hydriodic acid.	Ia.	127.6 "	—0.047 "	" 0.015 "	—0.037 "
	Ib.	"	—0.114 "	" 0.009 "	—0.089 "
	IIa.	157.2 "	—0.103 "	" 0.009 "	—0.066 "
	IIb.	"	—0.102 "	" 0.011 "	—0.065 "
	IIIa.	314.5 "	—0.177 "	" 0.008 "	—0.056 "
	IIIb.	"	—0.011 "	" 0.009 "	—0.003 "
Iodine and sodium sulphite.	Ia.	157.0 "	+0.105 "	" 0.006 "	+0.067 "
	Ib.	"	—0.031 "	" 0.012 "	—0.020 "
	IIa.	192.0 "	+0.002 "	" 0.014 "	+0.001 "
	IIb.	"	—0.127 "	" 0.012 "	—0.066 "
Chloral hydrate and potassium hydroxide.	a	201.0 "	+0.012 "	" 0.016 "	+0.006 "
	b	"	+0.007 "	" ——	+0.003 "
Chloral hydrate and water.		416.0 "	—0.003 "	" 0.009 "	——

It accordingly seemed to us worth while to make a similar set of weighings for another silver reaction which should be closely related to the one used by Landolt. The reaction used by us was the reduction of silver from an ammonia solution of the oxide by means of grape sugar, according to the formula, $Ag_2O + C_6H_{12}O_6 = Ag_2 + C_6H_{12}O_7$.

The method used was similar to the one employed by Landolt. The reagents were placed in separate sides of ∩ shaped glass tubes provided with side tubes for filling. The tubes were made as nearly as possible alike, of the best German glass obtainable, and had a capacity of about 250 cc. on each side, the connecting tube being only about one cm. in diameter.

The silver solution for each tube was prepared by dissolving 100 grams of silver nitrate in water, precipitating the silver oxide by ammonia gas, adding ammonia until the precipitate dissolved, and diluting with water to about 150 cc. This solution was placed in one side of the tube and a more than sufficient quantity of grape sugar solution to reduce the silver was placed in the other side. The total weight of the tube and contents was always in the neighborhood of 500 grams.

After two of the tubes were prepared in this way, their side tubes for filling were heated and drawn off, and the volumes of the two were compared by weighing them in water. An accessory glass tube was then prepared to be placed in the balance pan with the smaller tube, making the volume of the two equal to within less than one cc. to the volume of the larger tube. The tubes were then suspended from a balance, and their weights made equal to within one or two milligrams by first opening the larger tube containing the reagents and making its weight slightly greater than the combined weight of the other two tubes and permanently sealing it, after which the accessory tube was loaded with bits of fine wire until the equilibrium of the balance was restored, when it was also permanently sealed. As there was always a small loss of weight in sealing a tube, an additional weight of from five to twelve milligrams was required to balance the tubes against each other in weighing.

After the tubes were sealed, they were immersed in dilute sulphuric acid for several days, after which they were washed in distilled water and dried in an air bath at about 90 degrees.

The weighings were all made by Miss Ray, on a balance made to special order by Schickert, of Dresden, in 1891. The carrying capacity of the balance is 500 grams for each pan. It is provided with a mirror mounted upon the beam, and the deflections were observed by means of a telescope and vertical scale placed at a distance of 1.5 m. from the mirror. The balance was enclosed in a zinc case to insure uniformity of temperature, and was mounted upon a shelf supported by the heavy stone wall of a laboratory. The balance, as used, gave a deflection for one milligram of about 2.3 cm. as read by the telescope and scale.

The method of weighing was as follows : The resting point of the balance was determined three times, each time by using five successive turning points, and the mean of the three determinations was taken as the true resting point. The loaded tubes were then suspended from hooks above the balance pans, the accessory tube and the few milligrams of additional weight necessary to produce equilibrium were placed in the proper pan, and the mean of three more resting points was determined as before. The tubes were then removed, and the true resting point of the balance without load was determined as in the first case.

The difference between the resting point with the loaded tubes and the mean of the two true resting points without load gave the deflection due to the difference of the two loads, and this deflection divided by the sensibility of the balance gave a weight, p_1, which would, if placed in the proper pan, bring the balance to its true resting point.

The tubes were then suspended from the opposite arms of the balance, and a weight, p_2, which, if placed in the other pan, would bring the balance to its true resting point, was determined by the process used in the determination of p_1. The same additional weights were always used with the accessory tube, and in no case was a rider used upon the beam.

The balance arms were so nearly equal that p_1 and p_2 never differed by so much as 3 milligrams. It was found by computation that $A - B$, the true difference in weight of the tubes as calculated by the method of Gauss would in no case differ by as much as .001 mg. from the arithmetical mean of p_1 and p_2, hence in the computations $A - B = \frac{p_1 + p_2}{2}$.

The tubes, when not being weighed, were kept in a closed case which stood near the balance. They were handled with a large piece of chamois leather held in the hand, and were frequently wiped off with this leather. It was know that this method of handling would likely give rise to irregularities in the weight of the tubes, but since it seemed necessary to handle the tubes in some such way while thoroughly mixing the reagents, it was thought best to do so all the time, so that the mixing might be done under normal conditions.

A preliminary series of weighings was made in this manner with two tubes loaded with shot, the results of which are given below:

Set I. Tubes loaded with shot.
Weight of tubes 500 grams, approximately.

Date, 1896.	$A-B$.	Date, 1896.	$A-B$.
Nov. 6, P.M.	1.28 mg.	Nov. 25, P. M.	1.39 mg.
" " "	1.46 "	Dec. 2, "	1.33 "
" 12, A. M.	1.56 "	" 3, A. M.	1.41 "
" " P. M.	1.29 "	" 15, P. M.	1.46 "
" 13, "	1.38 "	" 17, "	1.54 "
" 19, "	1.28 "	" 18, "	1.53 "
" 20, "	1.33 "		

Mean value of $A - B = 1.40$ mg.
Probable error $= \pm 0.02$ mg.
Maximum dif. between two weighings $= 0.28$ mg.

While this series does not show the close agreement between individual weighings of the series made by Landolt, it still seemed to us to be close enough to detect any such change as was observed in his results.

The following sets of weighings were all made with tubes prepared in the way already described, and as nearly as possible alike. That they are not all complete is owing to a tendency of the tubes to crack where they had been sealed off. Sets II and V were spoiled by one of the tubes cracking before the reaction had taken place in either tube, and in Set III, one tube cracked when only two weighings had been made after the reaction in the first tube inverted.

In the data given below, the temperature in the balance case and the barometric height are given for each weighing. It will be seen that while some of the irregularities may be due to temperature changes, the barometric variations seem not to have affected the results.

SET III.

Date, 1897.	Temp.	Bar.	A–B.
Feb. 15, A.M.	18.6	30.15	1.75 mg.
" 17, "	16.1	30.07	1.79 "
" " P.M.	18.0	30.19	2.00 "
" 18, A.M.	16.0	29.66	1.85 "
" " P.M.	16.6	29.45	1.85 "
" " "	17.0	"	2.10 "
" 19, "	15.0	29.73	2.08 "
" 22, A.M.	15.0	30.28	1.89 "
" 23, "	13.0	30.43	2.00 "
" 24, "	14.9	30.38	1.90 "
" " P.M.	18.9	30.30	1.92 "
" 25, A.M.	16.0	30.27	2.04 "
" " P.M.	20.1	30.18	2.00 "

A–B=1.94±0.02 mg.

After Reaction in B.			
Feb. 29, A.M.	16.0	29.94	1.99 mg.
Mar. 3, P.M.	19.0	29.94	1.99 "

Apparent decrease in B=0.05 mg.

SET IV.

Date, 1897.	Temp.	Bar.	A–B.
Mar. 5, A.M.	17.3	29.98	2.26 mg.
" " P.M.	18.0	29.95	2.18 "
" 10, A.M.	15.8	30.30	2.24 "
" " P.M.	18.2	30.25	2.22 "
" 11, P.M.	17.7	30.19	2.10 "
" 12, A.M.	16.0	30.20	2.31 "
" " P.M.	17.8	30.16	2.22 "

A–B=2.22±0.005 mg.

After Reaction in B.

Mar. 15, A.M.	16.0	29.96	2.10 mg.
“ 16, “	19.0	29.92	2.23 “
“ 17, “	17.0	30.03	2.16 “
“ 18, “	19.0	30.00	2.08 “
“ “ P.M.	19.0	30.00	2.59 “
“ 19, A.M.	17.2	30.08	2.38 “
“ “ P.M.	19.0	30.05	2.32 “

A–B=2.27±0.044 mg. Apparent increase=0.05 mg.

After Reaction in A.

Date, 1897.	Temp.	Bar.	A-B.
Mar. 20, A.M.	16.	30.14	2.37 mg.
“ “ “	16.	30.14	2.10 “
“ “ P.M.	16.9	30.15	2.29 “
“ 30, A.M.	14.5	30.35	2.26 “
“ 31, “	17.	30.16	2.34 “
“ “ P.M.	19.	30.09	2.39 “
Apr. 1, A.M.	16.	30.21	2.35 “

A–B=2.30±0.025 mg. Apparent increase=0.03 mg.

SET VI.

Date, 1897.	Temp.	Bar.	A-B.
Apr. 21, A.M.	21.	30.25	2.35 mg.
“ “ P.M.	19.	30.28	2.54 “
“ 22, A.M.	21.2	30.15	2.33 “
“ “ P.M.	19.	30.10	2.54 “
“ 23, A.M.	18.1	30.20	2.42 “
“ “ P.M.	21.	29.97	2.52 “
“ 24, A.M.	19.2	30.50	2.64 “
“ 25, P.M.	24.	30.1	2.28 “
“ 26, A.M.	21.	29.95	2.34 “

A–B=2.44±0.026 mg.

After Reaction in B.

Apr. 27, A.M.	18.	30.60	2.47 mg
“ 28, “	18.4	30.21	2.39 ‘
“ “ P.M.	19.9	30.17	2.37 “
“ 29, A.M.	18.	30.14	2.45 “
“ “ P.M.	20.3	30.10	2.32 “

A B=2.40±0.018 mg. Apparent increase=0.04 mg.

After Reaction in A.			
Apr. 30, A.M.	19.	30.06	2.49 mg.
" " P.M.	21.	"	2.58 "
May, 3, A.M.	20.5	29.94	2.42 "
" 4, "	19.	30.10	2.42 "
" 5, "	19.	30.12	2.59 "
" " P.M.	21.	30.15	2.60 "
" 7, A.M.	19.	30.15	2.43 "
" " P.M.	20.	30.08	2.34 "

A–B=2.48±0.023 mg. Apparent increase=0.08 mg.

The following table gives a summary of the results:

No. of Experiment.	Wt. of Reagents.	Observed Change.	Prob. error.
IIIa.	120.9 g.	—0.05 mg.	±0.02 mg.
IVa.	"	—0.05 "	±0.049 "
IVb.	"	+0.03 "	±0.069 "
VIa.	"	+0.04 "	±0.044 "
VIb.	"	+0.08 "	±0.04 "

After the weighings were completed, one tube from Set IV and one from Set VI were opened, and the amount of reduced silver was determined. This determination gave for the tube from Set IV, 59.7 grams, and for the tube from Set VI, 61.9 grams.

In Landolt's determinations, the amount of reduced silver in experiment I was 38.2 grams. In experiment II it is not given, but supposing the reduction to have been complete, there would have been 60.03 grams. The average loss of weight for 100 grams of reduced silver was, accordingly, 0.32 mg. A corresponding loss in our weighings would have shown a change of 0.192 mg. after each reaction. This is more than four times the probable error of our weighings, while the average change in weight observed by us was not greater than this probable error, and was sometimes positive and sometimes negative.

It would seem from the above, that while our weighings show much greater irregularities than those of Landolt, they are still accurate enough to make it extremely improbable that the reaction used by us was accompanied by any such change in weight as was observed in the similar reaction used by him.

Stanford University, May 20, 1897.

NOTE.

The American Association for the Advancement of Science.—The forty-sixth annual meeting of the A. A. A. S., held at Detroit, Aug. 9–14, was distinctively a working meeting. The general attendance was small, yet the sessions of the Physics, Section B, were always well attended by a particularly representative membership. The regularity in this attendance contributed noticeably to the pleasure of the meeting, as did also the absence of the drifting element and those only superficially interested. The papers were above the average, both in number and quality; but although the programme was strong, the discussions were notably weak. The housing of all sections in one commodious building, the Central High School, added materially to the comfort of the meeting. Less time than usual was devoted to social functions. The time lost by many of the executive officers of the Association on account of the old-fashioned organization being but poorly adapted to modern needs, is to be regretted, it is hoped that a remedy may be found by modifying the methods for transacting routine business.

The officers of the Physical section were Carl Barus, Vice-President and Chairman of the section; Frederick Bedell, Secretary. For his opening address to the section, Vice-President Barus selected the subject "Long Range Temperature and Presssure Variable in Physics," after which thirty-eight papers were presented as follows:

The Design, Construction and Test of a 1250 Watt Transformer, by Henry S. Carhart.—Electrolytic Action in a Condenser, by K. E. Guthe.—Stereoscopic Views of Spherical Catenaries and Gyroscopic Curves, by A. G. Greenhill.—The Treatment of Differential Equations by Approximate Methods, by W. F. Durand.—A new Method of Solving Certain Differential Equations that occur in Mathematical Physics, by A. Macfarlane.—The Magnetic Survey of Maryland, by L. A. Bauer.—The Transmission of Radiant Heat by Gases at Varying Pressures, by Charles F. Brush.—On the Rate at which Hot Glass absorbs Superheated Water, by Carl Barus.—A New Method of determining the Specific Heats of Liquids, by Robert L. Litch.—On the Coëfficient of Expansion of Certain Gases, by Edward W. Morley and Dayton C. Miller.—The Effect of Heat on the Elastic Limit and Ultimate Strength of Copper Wire, by Frank P. Whitman and Mary C. Noyes.—A Method of obtaining Capillary Canals of Specified Diameter, by Carl Barus.—Kites and their Use by the Weather Bureau in Explorations of the Upper Air, by C. F. Marvin.—Arc Spectra, by Arthur L. Foley.—On the Brightness of Pigmented Surfaces under Various Sources of Illumination, by Frank P. Whitman.—Note on the Construction of a Sensitive Radiometer, by Ernest Fox Nichols.—Photographs of Manometric Flames, by Edward L. Nichols and Ernest Merritt.—The Discharge of Electrified Bodies by X-rays, by C. D. Child.—The Electric Conductivity of certain Specimens of Sheet Glass, with reference to their Fitness for Use in Static Generators, by Dayton C. Miller.—Graphical Treatment of Alternating Currents in Branch Circuits in case of Variable Frequency, by H. T. Eddy.—On Simple Non-Alternating Currents, by Alexander Macfarlane.—Exhibition of Instruments for determining the Frequency of an Alternating Current, by George S. Moler and Frederick Bedell.—The Predetermination of Transformer Regulation, by F. Bedell, R. E. Chandler and R. H. Sherwood, Jr.—The Effect of Pressure on the Wave-lengths of the lines of the Emission Spectra of the Elements, by W. J. Humphreys.—A New Form of Coal Calorimeter, by Charles L. Norton.—Notes on the Recent History of Musical Pitch in the United States, by Charles R. Cross.—A New Form of Harmonic Analyzer, by Frank A. Laws.—The Determination of the Surface Tension of Water, and of Certain Aqueous Solutions, by Means of the Method of Ripples, by N. Ernest Dorsey.—The Series of International Cloud Observations made by the U. S. Weather Bureau, and their relation to Meteorological Problems, by Frank H. Bigelow.—The Effects of Tension and Quality of the Metal upon the Changes in Length produced in Iron Wires by Magnetization, by B. B. Brackett.—The Measurements of Small Gaseous Pressures, by Charles F. Brush.—An Electrical Thermostat, by W. R. Whitney.—An Apparatus for Testing the Law of Conservation of Energy in the Human Body, by W. O. Atwater and E. B. Rosa.—Electrical Resonance and Dielectric Hysteresis, by Edward B. Rosa and Arthur W. Smith.—On the Electrostatic Capacity of a Two-wire Cable, by G. W. Patterson, Jr.—A method for the Determination of the Period of Electrical Oscillations and Other Applications of the Same, by Margaret E. Maltby.—The influence of Time and Temperature Upon the Absolute Regidity of Quartz Fibres, by Samuel J. Barnett.—On the Methods of Measuring Mean Horizontal Candle Power, by C. P. Matthews.

The Association next meets at Boston, August 22, with the following officers: F. W. Putnam, President; L. O. Howard, Permanent Secretary; D. S. Kellicott, General Secretary; F. Bedell, Secretary of Council; F. P. Whitman, Vice President and Chairman, Section B.; E. B. Rosa, Secretary Section B.

NEW BOOKS.

The Mechanics of Pumping Machinery. By Julius Weisbach and Gustav Herrmann. Translated from the second German edition by Karl P. Dahlstrom, M.E. 8vo, pp. 300. The Macmillan Company, New York, 1897.

The modern text-books on pumping machinery accessible to the English reader are mainly descriptive of types and of mechanical details. The student of theory has recourse to the German works of Redtenbacher, Meissner and Weisbach or to the French treatises of Gérardin, Berthot, Bresse or Collignon for satisfactory analytical discussions.

The translation of this portion of Weisbach's classic work will therefore be welcomed by those who feel the need of a book of reference on pumping machinery which shall unite, with the descriptive matter a satisfactory demonstration of the theory involved. The characteristic of Weisbach's writings—his constant leaning toward practical applications of theory to the problems of ordinary practice—is common to this portion of his work. The book will be especially acceptable to those desiring the solution of concrete examples.

Chapter I. is mainly of historic interest and deals with the earlier forms of water elevators. In Chapter II. considerable space is devoted to the different forms of valves and pistons and to the mechanical action of reciprocating pumps whose theory is discussed in chapter III. The translator has added modern data to his description of pump types. Weisbach does not treat fully the theory of rotary pumps, but Redtenbacher's well-known treatise on turbines covers this field. Buchetti's small work, published in 1895, gives methods of computing the dimensions for actual designs.

Among the other forms of water raising machinery it is interesting to find a description of the pulsometer now much used for small lifts because of its adaptability to the diverse conditions existing about open excavations.

Elon Huntington Hooker.

Deductive Physics. By Frederick J. Rogers. 8vo, pp. vi + 260. Ithaca, N. Y., Andrus and Church, 1897. (*Received.*)

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ELECTRICAL ENGINEERING DEPARTMENT
University of Illinois.

THE

PHYSICAL REVIEW.

THE EFFECTS OF TENSION AND QUALITY OF THE METAL UPON THE CHANGES IN LENGTH PRODUCED IN IRON WIRES BY MAGNETIZATION.

By Byron B. Brackett.

Historical.

FIFTY years ago a machinist of Manchester imagined that he could see the volume of a mass of iron increase when it was magnetized and decrease when the magnetizing force was removed. Hoping to be able to use this principle in the construction of an electro-magnetic engine, he appealed to Dr. Joule[1] to investigate the phenomenon and determine the amount of the change. By immersing the iron to be magnetized in a closed vessel, filled with water, in which stood a fine capillary tube Joule could not detect any change of volume, though it has since been shown[2] that had he used either stronger or weaker fields he probably would have done so. But by a system of compound levers of great multiplying power he proved that an iron bar did change it length when magnetized longitudinally. He observed an increase in length of 1–200,000. As a result of his investigations he proposed the following laws:

1. When soft iron rods are magnetized their length increases

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[1] Joule, Phil. Mag., (3), vol. 30, pp. 76, 225.
[2] Bidwell, Proc. Roy. Soc., vol. 56, p. 94.

and the elongation is approximately proportional to the square of the magnetizing force.

2. Tension applied to the rod diminishes the elongating effect.

3. The elongation is greater for the same intensity of magnetism in proportion to the softness of the metal.

That the two first laws are correct for the fields that Joule used cannot be doubted; but as to the third law there is much uncertainty. The investigations of Shelford Bidwell[1] indicate that not only hardening, but also annealing an iron rod diminishes the elongating effect, and at the best the relation between the softness of the iron and its change of length is to-day very much confused.

It was nearly twenty-five years after Joule's investigations before the question was taken up again experimentally by Barrett[2] and nearly at the same time by Mayer.[3] Barrett employed the tilting mirror, which is described under the apparatus used in this investigation, a device suggested to Barrett by Professor Rowland. He experimented not only upon iron, but also upon nickel and cobalt.[4] He observed an elongation of 1–260,000 for iron and 1–425,000 for cobalt; and a contraction of 1–130,000 for nickel.

Mayer found an elongation of 1–277,000 for iron. Some of his observations upon the action of hard steel seemed to be at variance with Joule's results. But Bidwell has since shown that this apparent difference was due solely to their different methods of experimenting.

In 1885 Bidwell[5] reported the first of a very extensive series of experiments upon the distortions caused by magnetization in iron, nickel and cobalt. He carried his investigations up to fields many times stronger than those used by the earlier investigators. He has worked upon the effects of tension, tempering and annealing. He has experimented with both rods and rings. He found that, at least with his apparatus, rods did not continue to elongate, but reached a minimum length, then gradually shortened until they had less than their initial length, apparently approaching a limiting value asymptotically. Investigations upon this subject have also been made by Berget,[1] Bock,[2] Jones,[3] Knott,[4] Lochner,[5] Nagaoka ;[6] and two years ago investigations in this line were begun here, in the Johns Hopkins laboratory, by Dr. L. T. More.[7]

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[1] Bidwell, Proc. Roy. Soc., vol. 55, p. 228.

[2] Barrett, Phil. Mag., 1874, vol. 47, p. 51.

[3] Mayer, Phil. Mag., 1873, vol. 45, p. 350; Mayer, Phil. Mag., 1873, vol. 46, p. 177.

[4] Barrett, Nature, 1882, vol. 26, p. 585.

[5] Bidwell, Proc. Roy. Soc., 1885, vol. 38, p. 265; 1886, vol. 40, pp. 109, 257; 1888, vol. 43, p. 407; 1890, vol. 47, p. 469; 1892, vol. 51, p. 495; 1894, vol. 55, p. 228; 1894, vol. 56, p. 94; Phil. Trans. Roy. Soc., 1888, vol. 179 (A), p. 205.

At Professor Rowland's suggestion, Dr. More determined the intensity of magnetization in his wires for each change of length observed, and also sought to take into consideration the secondary actions that might affect the length of the wire.

Professor Rowland defends his position on the subject as follows :

The change in length may be partly due to other causes than the magnetization of the metal. Among these one can put the stresses due to magnetization. It is not at all evident that these stresses can be exactly identified with the Maxwellian stresses. If we think of the long wire that More used as composed of a bundle of small elementary magnets tied together at points well inside the poles, the magnets would seem to have no tendency in their central parts to separate from each other; and in that case there would be no pressure at right angles to the lines of induction, unless it can be shown to result from a squeezing outwards of some kind of matter caused by a longitudinal compression.

At the same time, the compressive force which, it is known, will tend to close up a very thin air-gap in a divided magnet must also exist in any magnet, for, according to all our ideas of matter, there is no real difference in the case where the air-gap exists and where it does not; because we still must consider the gaps between the molecules. If we now think of the long elementary magnets as composed of short elementary pieces with their ends so near together that the effects of their poles are neutralized in all action upon external bodies and yet allow a space between their ends for a compressible medium to entirely surround them, then this longitudinal pressure would cause both longitudinal shortening and a pressure outwards perpendicular to the induction. It would seem that such a case might represent both the strain in the medium and the strain in the ether.

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[1] Berget, Comp. Rend., tom. 65, p. 722.

[2] Bock, Wied. Ann., 1895, vol. 54, p. 442.

[3] Jones, Phil. Mag., 1895, vol. 39, p. 254.

[4] Knot, Phil. Mag., 1894, vol. 37, p. 141 ; Proc. Roy. Soc. Edinburg, vol. 18, p. 315 ; vol. 20, p. 290; vol. 20, p. 334.

[5] Lochner, Phil. Mag., 1893, vol. 36, p. 504.

[6] Nagaoka, Wied. Ann., 53, pp. 481, 487 ; Phil. Mag., 1894, vol. 37, p. 131 ; 1896, vol. 41, p. 454.

[7] More, Phil. Mag., 1895, vol. 40, p. 345 ; Phys. Rev., 1895, vol. 3, p. 210.

The value of this compressive force is probably $\frac{B^2}{8\pi}$. It may possibly be $\frac{(B-H)^2}{8\pi}$, but in most cases the two are so nearly equal in value that it would not seriously alter the results to take either force. Taking this as equivalent in its action to a simple mechanical pressure and considering, with it, the ordinary elasticity of the wire, the shortening due to this cause can be computed for each observation and corrections may be made accordingly. Or this force might be considered to neutralize a portion of the tension on the wire equal to it in value.

For reasons like these, Professor Rowland advised Dr. More to correct his observed readings of elongation for a shortening caused by the force $\frac{B^2}{8\pi}$; and these corrected readings were plotted to a basis of induction in the iron instead of being given on a basis of the magnetizing force, as all previous curves in this subject had been given, except those of Nagaoka.

Last year Dr. E. F. Gallaudet[1] made an investigation on this subject in this laboratory. He used the apparatus employed by Dr. More, and his curves were in all respects except one similar to those obtained by Dr. More, Bidwell and others. He did not correct them for a contraction caused by a force of $\frac{B^2}{8\pi}$.

An initial contraction was observed by him that no one has announced before, and he also gave observations that seemed to indicate a very great change in the values of Young's modulus as the magnetization increased. That there could be no real change in the elasticity so large as these, can be shown from Dr. Gallaudet's own tables; for such changes in value of Young's modulus under the larger tensions used would have caused changes of length in the wire many times greater than the changes observed. Moreover, Bock[1] found as a result of his experiments that the changes in elasticity due to magnetization could not exceed one-half per cent.; and Miss Noyes,[2] in a series of experiments upon the changes of Young's modulus with temperature, did not find any evidence of a change of the elasticity from magnetization.

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[1] Gallaudet, J. H. U. Thesis, June, 1896.

OBJECT OF THIS INVESTIGATION.

It was hoped that by carefully observing the behavior of quite a number of different wires under varied physical conditions data might be obtained that would explain the relation of the elasticity of the metal to its change in length under magnetization. It was also hoped that incidentally the question of an initial contraction preceding elongation, as observed by Dr. Gallaudet, might be settled; and that likewise some additional information on the relation of the temper of the metal to its change of length might be obtained.

APPARATUS.

I employed the apparatus used by Dr. More and by Dr. Gallaudet. It is very fully described by Dr. More, who[3] gives all the dimensions essential to its construction. The feature that is essentially characteristic of this apparatus, in addition to Professor Rowland's tilting mirror that both Barrett and Bidwell used, is the cylindrical jacket which was suggested by Dr. Ames. The figure here given will show its use. About the part of the wire where the changes in length are to be observed is placed a brass tube having a free internal diameter of over one centimeter. At the bottom end *b* the tube is clamped firmly to the wire while the upper end has a loosely fitting cork merely to keep the axis of the cylinder and the wire coincident. To the top of the cylinder is attached a projecting arm that carries two raised supports *a* and *c*. Above this arm is placed a lever *d*, resting by a knife-edge on the support *a*. This lever also has an inverted knife-edge but a very short distance back of the one that supports its weight, and over the ininverted knife-edge is placed a hook that is firmly clasped to the wire at *g*. Thus any change in the length of the wire between the points *b* and *g* will cause the outer end of the lever to rise or fall. Standing partly on the end of this lever and partly on the support *c* is a little table having three needle-point legs and carrying a vertical plane mirror. Now any movement of the lever arm relative to the support *c* will cause the mirror to tilt; and by observing with a telescope at some distance the image in the mirror of a ver-

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[1] Bock, *loc. cit.*

[2] Mary C. Noyes, Phys. Rev., 1896, vol. 3, p. 432.

[3] More, *loc. cit.*

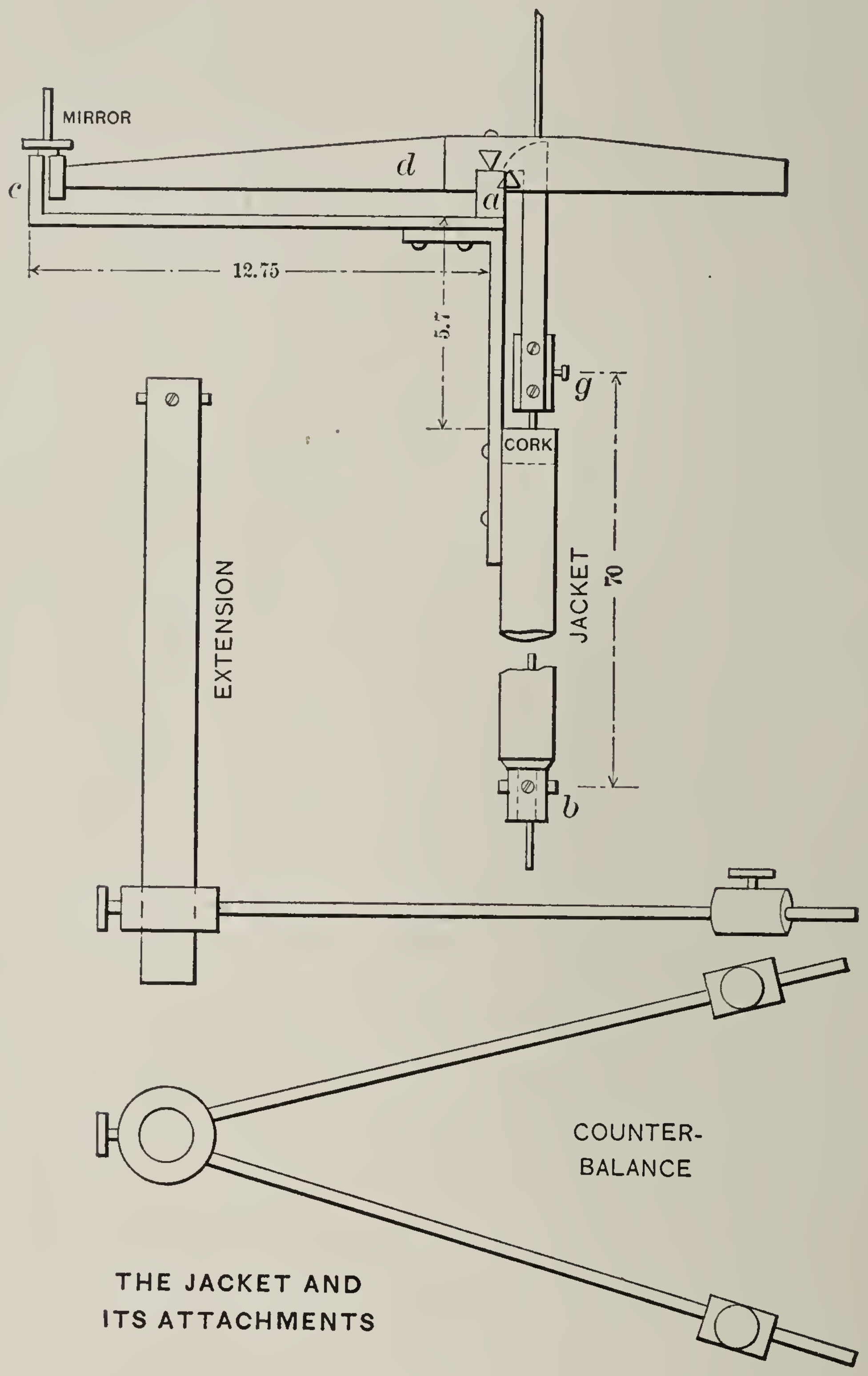

Fig. 1.

tical scale the movement of the lever arm will be greatly multiplied.

If,

$L =$ length of the long arm of the lever,

$l =$ length of the short arm of the lever,

$d =$ distance from one leg of the brass table to the line joining the other two legs, resting on lever end,

$D =$ scale distance from the mirror, then the multiplying power of the apparatus is

$$\frac{L}{l} \times \frac{D \times 2}{a}.$$

$L = 11.672$
$l = 0.4776$
$d = 0.3335.$

D differed slightly for different tests, but was nearly always as great as 170. Thus the multiplying power of the apparatus was always approximately 25,000.

To determine the elongation caused by the tensions used in most of the tests, and to check the values of the modulus of elasticity determined from adding small weights, the mirror at c was removed, and another mirror was attached to the lever d just above the point a, so that the apparatus acted as a simple multiplying lever. This arrangement gave a multiplying power of only $2\frac{D}{l}$, or about 700, and so was capable of being used for much greater elongations.

The wire to be tested is always suspended from a point some distance above the apparatus in such a way that the jacket has its ends well within the magnetizing solenoid. The solenoid rests upon a support entirely independent from that of the wire and those parts of the apparatus attached to the wire; and the two parts are so adjusted that there is no contact between them.

I made but two changes in the apparatus itself that could be supposed to affect the results. In the apparatus as used by Dr. More and by Dr. Gallaudet, the jacket and its attachments were not perfectly balanced. The lever d was as nearly balanced as was desirable to have it. But all the weight of the lever came on one side of the wire; and that together with the weight of the arm under it, gave quite a tendency to the jacket to tip sidewise and slightly bend the wire. Then the amount of this bending would doubtless be changed by the magnetic stresses; and especially when no weight was attached to the lower end of the wire, it seemed that this might possibly have quite an effect upon changes so small as those to be observed. An adjustable extension was therefore made to attach to the lower ends of the jacket, extend well down below the magnetizing solenoid, and by means of sliding weights on its two arms exactly counterbalance the moment given the jacket by the arm at the top, together with the lever and mirror. Again, the jacket had been clamped, at its bottom, to the wire by a single set-screw. This it seemed might cause a bend in the wire. The brass plug *b* was therefore bored out quite a little larger than the wire. Two extra radial set-screws were put into *b*, making three equally spaced ones about the circumference. Then a little cylinder of brass, slit lengthwise on one side, bored along its axis to fit the wire and just large enough to slip into the hole in *b*, was put about the wire at the point of attachment. In this way by the use of the three screws the wire could be accurately centred in the jacket.

These precautions seemed necessary, for a slight, almost imperceptible bend in the wire was found by actual experiment to greatly modify the readings obtained. These adjustments were so delicate that it was scarcely possible to judge when they had been properly made except by actual trial. After Young's modulus had been carefully determined, it was considered a fair test of the adjustment of the apparatus to put on and off a weight that ought to cause changes in length approximately equal to those expected to follow from magnetization. If the computed readings were obtained, the adjustment was assumed to be correct.

A most evident source of error in this work is the change of length caused by temperature. The greatest value of $\frac{dl}{l}$ observed in my investigation which seemed to be due to magnetization was 33.92×10^{-7}. A change of one degree centigrade in the temperature of the wire would give for $\frac{dl}{l}$ about $120. \times 10^{-7}$. To prevent temperature changes the magnetizing solenoid was made originally from two coaxial brass cylinders in such a way as to leave a space for water between them. In all the experiments of this investigation water was being continually forced into this space from the bottom and was overflowing at the top. Thus there was a jacket of continuously changing water between the magnetizing coils and the jacket attached to the wire. Yet if the larger currents were left on even for a short time the effects of temperature changes were quickly visible. If the conditions have become steady and all the apparatus has attained a constant temperature, and then a current is put on and allowed to remain, the resulting higher temperature first affects and expands the jacket on the wire, causing an apparent *contraction* of the wire itself, and as the higher temperature reaches the wire *expansion* will be observed until at last, in the steady state resulting, the wire will have apparently expanded or contracted according to the relative coefficients of heat expansion for the wire and for the jacket. Fortunately, these changes are comparatively slow and can be separated with a limited degree of accuracy from readings that can be taken quickly. But if one were to take a series of readings from the modulus of elasticity, using small changes of weight while the magnetizing current remained on, it might be foreseen that the results would apparently vary first in one direction and then in the other, due to the unequal changes in the length of the jacket and the wire caused by the heating.

Incident to the great multiplying power of the apparatus, the slightest mechanical vibration of the wire under test made it absolutely impossible to read the changes of length accurately, and, in many cases, to read the figures of the scale at all. To get a support for the wire as free from vibrations as possible, a weight of some seventy-five or eighty pounds was made by filling a wooden box with bricks and old storage battery plates, and this was suspended by a single coiled brass spring to an arm projecting from a side-wall of the room some ten feet above the apparatus. The spring was so designed that, with the weight it carried, its period of vibration was very slow, and when the wire to be tested was suspended from the bottom of the weight very little trace of the ordinary vibrations of the building ever reached the wire. But to have the system work perfectly, it was necessary that the wire and the weight which it carried be attached exactly under the center of mass of the large weight above. To facilitate this adjustment four projecting arms were attached to the bottom of the box forming the weight and on these arms were hung small movable baskets of shot. When all was properly adjusted the scale image in the telescope would be blurred for a half-minute by a wagon passing on the cobble-stone pavement some twenty or thirty feet from the apparatus, but otherwise all was undisturbed.

General Method of Working.

It was decided that, for each kind of iron tested, series of elongation readings should be taken for several different tensions, that the elongations should be observed at each point both with the magnetizing field on and with the field off, and that for all observations of elongation the corresponding inductions in the iron should be obtained.

When the wire had been properly adjusted in the apparatus, with a weight attached to the lower end of the wire to give the desired tension and after the test with the small weight already mentioned had been made, the first elongation reading was taken by sending a very weak current through the magnetizing solenoid, quickly observing the scale reading in the telescope and then breaking the current and reading the telescope again. This procedure was repeated with gradually increasing values of the current until the maximum current was reached. The elongation readings obtained with the field on, I shall call the "total" elongation, since it represents both the temporary and the permanent or residual change in length. The elongation observed after a certain magnetizing field has been put on and then taken off, will be called the "residual" elongation for that definite field.

All the currents used in magnetizing were left on only long enough to get the required readings accurately. A very careful watch was kept for temperature changes and when they began to appear, the readings were stopped until all had cooled down to the steady state again. The currents used were measured by carefully tested Weston instruments of different capacity according to the currents to be read.[1]

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[1] See end of article for notation used in tables and plates.

The elongation data given to this investigation were all taken the first time the wire was magnetized after it had been put into the apparatus. No method seemed efficient in completely demagnetizing the wire while it was in place in the apparatus. Alternating currents would not do it, probably because the ends of the wire extended so far beyond the solenoid. After using the alternating current, the ends of the wire would still show a decided polarity of the same kind that it showed before, and the effects upon the elongation curve were very marked. If the magnetizing field was applied again in the same direction as before the curve was very similar to the one obtained at first though the amount of the elongation was somewhat reduced. But if the field was applied in the opposite direction the first effect with weak fields was a contraction, followed by a decrease of the contraction, then by elongation and the rest of the curve was like other curves for the same wire.

The inductions corresponding to the elongations were obtained from a separate series of readings by means of the ballistic galvanometer, using the method of increasing reversals and breaking the current to get the difference between the total and residual inductions. It was exactly the method used by Professor Rowland[1] in his well known ring experiments. Though actual test had shown the field to be practically uniform along that part of the solenoid where the elongation of the wire was measured, yet, as a precaution against irregularities in the induction of the wire, the test coil was distributed in four equal parts equally spaced along the portion of the wire under observation. For the lower inductions seven hundred turns were used, while for the higher inductions this number was reduced to four hundred turns. The galvanometer with its circuit was calibrated for each connection; and, moreover, the ratio of the deflections obtained by the two different arrangements was always taken each time the change was made.

With soft iron wires the alternate current was applied after the elongation readings had been taken and the induction readings were then made from the same wire. While the elongation curve would have been quite different under the second magnetization, yet actual test showed that the first induction curve taken for any wire did not differ from subsequent ones. For the hard wires tested the induction curves were taken from different wires apparently exactly like the ones used for the elongation tests.

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[1] Rowland, Phil. Mag. (4), vol. 46, p. 140.

WORK DONE.

During my investigation I made not less than thirty-five elongation tests. All of the more important series of readings were repeated, with different wires, several times, in order that none of the curves should represent chance results. No series given in the tables rests upon the evidence of a single experiment. Tests are reported upon piano-wire in its natural condition under two different stresses, annealed piano-wire under three different stresses and soft annealed iron wire under four different stresses, making nine series in all.

The lower tension used for natural piano wire was the smallest that would apparently free the wire from bends, for it could not be obtained in a perfectly straight form. The larger tension was a little more than half way to the elastic limit. These wires have a diameter of 1.25 mm. The same wires used in these tests were then heated to a bright red by passing a current through them and were allowed to cool slowly in the air. The slightly burned outside of the wires was then carefully removed by the use of fine flint-paper, after which they were tested under the same tension as before. Since the wires came out of the annealing process perfectly straight it was possible with these wires to make a test under the tension caused by the apparatus alone.

But neither with the natural nor annealed piano wire were the changes caused by magnetization very great, or the effects of a permanent strain especially marked. The most extensive investigation was therefore made upon very soft annealed iron wire. The different pieces were cut from one continuous piece, and parts taken out at different places were analyzed by Mr. Nakaseko of this university, and were pronounced by him to be very pure and quite free from carbon as well as from all other impurities. All of these wires were heated to the bright red state just as in the case of the annealed piano wire, but here it was only to free them from kinks. They gave the same magnetization curve before and after this process ; and while the same elongation curve could not be obtained in the two cases, all of my work would lead me to think that the difference was due solely to the bends in the wire before the heating process.

This wire as tested had a diameter at 1.31 mm. Its behavior was investigated under five different tensions, ranging from that due to the apparatus only up to the elastic limit of the wire. Under the largest tension the wire would elongate the usual amount upon the addition of a very small weight, but would require quite a little time to recover when this weight was removed. Possibly because the elastic changes were so slow under this tension, it was impossible to take both the total and residual elongation curves. The best that could be done was to keep the current on all the time and correct as well as could be done for the temperature changes to obtain the curve for total elongation.

Not only were more tests made on this kind of iron, but the readings obtained have been worked out, somewhat more fully. Elongation curves from these results have been plotted both to H and to I, and in both cases the contractions that Professor Rowland believes will be caused by the force $\frac{B^2}{8\pi}$ have been plotted in a separate curve and the elongation curve as modified by this contraction has also been given. For the residual curves this supposed contraction is computed from $\frac{(4\pi I)^2}{8\pi}$, the residual value of I, of course, being taken.

The value of Young's modulus used in computing the contraction curves for this iron was 2.12, and was determined both from small changes of tension, using the higher multiplying power and from larger changes of tension using the lower multiplying power. The latter method was considered the more reliable, especially in determining the elongations due to large tensions. But this method could not be used to investigate the possibility of a change in modulus caused by magnetization, since it required so much time to adjust the weights that temperature effects from the magnetizing current would surely modify the results. By using the smaller changes of tension, no variation in the modulus could be detected at any stage of magnetization, except such as were within the limits of error of the method.

It was desired to take the induction readings for exactly the same values of the magnetizing current that had been used in the elongation tests; but on account of the bad condition of the storage batteries that I had to use during most of my work, I could only make the currents approximately equal in the two cases. The points on each curve, especially where there could be any doubt as to the exact location of that curve, were taken very close together; and then through interpolation on the curves, the corresponding values of elongation and induction could be found with as great accuracy as the curves could be plotted.

General Results.

Speaking very generally, all the curves of elongation when plotted to H show either no change of length up to nearly the field at which the magnetic saturation occurs or up to that point their elongation curve is similar in shape to the induction curve. Not far beyond this the residual curves become practically horizontal and show no further change in length of any consequence; while the curve of total elongation begins at nearly the same point to descend along an approximately straight line. The residual curves for soft iron seem to fall quite a little after the higher fields have been applied. But while there seems to be no doubt that a slight shortening of the wire occurs, due probably to a rearrangement of the molecules under the strain of the higher fields, yet, I am sure that this shortening is exaggerated in my readings by temperature effects that could not be avoided. All the changes of length due to any change in the magnetic conditions are very quick; and the almost instantaneous throw of the lever and mirror from one position to another, when quite large, as it was here, caused vibrations that required a little extra time to die out, and consequently allowed some heating of the jacket to occur. All attempts to damp this vibration involved the possibility of displacing parts that must move so freely as the lever and the mirror; and to put the current on and off gradually involved about the same possibility of heating as did the time required for the vibrations to cease. It thus seemed best to make these readings as well as could be done and then give them with the above caution. In some cases I have drawn the ends of the curves somewhat above the plotted points, thinking them much more nearly correct there than if drawn through the points. For the same reason I think it probable that the ends of the total elongation curves should show a marked tendency to curve upwards from the straight line, as has been found by Bidwell.[1]

EFFECT OF THE FIELD.

In any case the residual curve shows that for quite a distance beyond the point of saturation no permanent change of length occurs; and the curve of total elongation shows, through these same field values, a contraction directly proportional to the field strength. Thus these two curves, at least along this portion, seem to show clearly that there is an elongation due directly to the induction in the iron and a contraction caused by the field, directly proportional in value to the field strength, as has been suggested by Dr. Galaudet.[2] Every set of curves given in this investigation seems to show a contraction due to the magnetizing field. Where plotted to I, the elongation, in any one test, is always greater for the same value of induction when the field is off than when it is on; and the curves modified by the supposed contraction due to $\frac{B^2}{8\pi}$ and $\frac{(4\pi I)^2}{8\pi}$, both when plotted to H, and when plotted to I, indicate the same thing.

Increased tension on the wire apparently causes the contraction to begin at a lower field and at a lower induction in the iron; but when the elongation reaches the straight line portion of the total elongation curve plotted to H, it continues on the same slope without any regard to the tension upon the wire. In other words the straight portions of all these curves for the same kind of wire are parallel. For another kind of wire the slope of this portion of the curve will be entirely different; but here again the curves for this wire, taken under different tensions, will have parallel parts. This would seem to indicate that the contraction caused by the field is not only proportional to the field, but that it depends also upon some definite constant for each kind of iron. For the three different kinds of iron that I have studied these constants would have to be about in the ratio of 12.5, 18 and 29 for the natural piano wire, annealed piano wire, and the soft iron respectively. It does not seem possible to identify these values directly with either the elasticity or the magnetic permeability of the wires. The curves as modified by the $\frac{B^2}{8\pi}$ correction would have a slightly smaller absolute value for these constants, but the ratio of their values would be very nearly the same.

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[1] Bidwell, *loc. cit.*

[2] Gallaudet, *loc. cit.*

Looking at the total elongation curves plotted to I, it will be seen that the retraction of length does not appear until the value of μ becomes very much reduced, or until a relatively large increase of the field is necessary for a small increase of induction in the wire. As the field necessary to give a certain increase of induction becomes larger and larger, the curves show a more and more rapid contraction. Since this part of the curve gradually becomes more and more nearly vertical it seems evident that it must approach asymptotically the vertical line drawn through the maximum or limiting value of I, which for this soft iron would doubtless be about 1700.[1] This would indicate that there is no limit to the contraction which is caused by the field, but the contractions resulting from a definite amount of field increase might finally become less and less, as Bidwell[2] has observed.

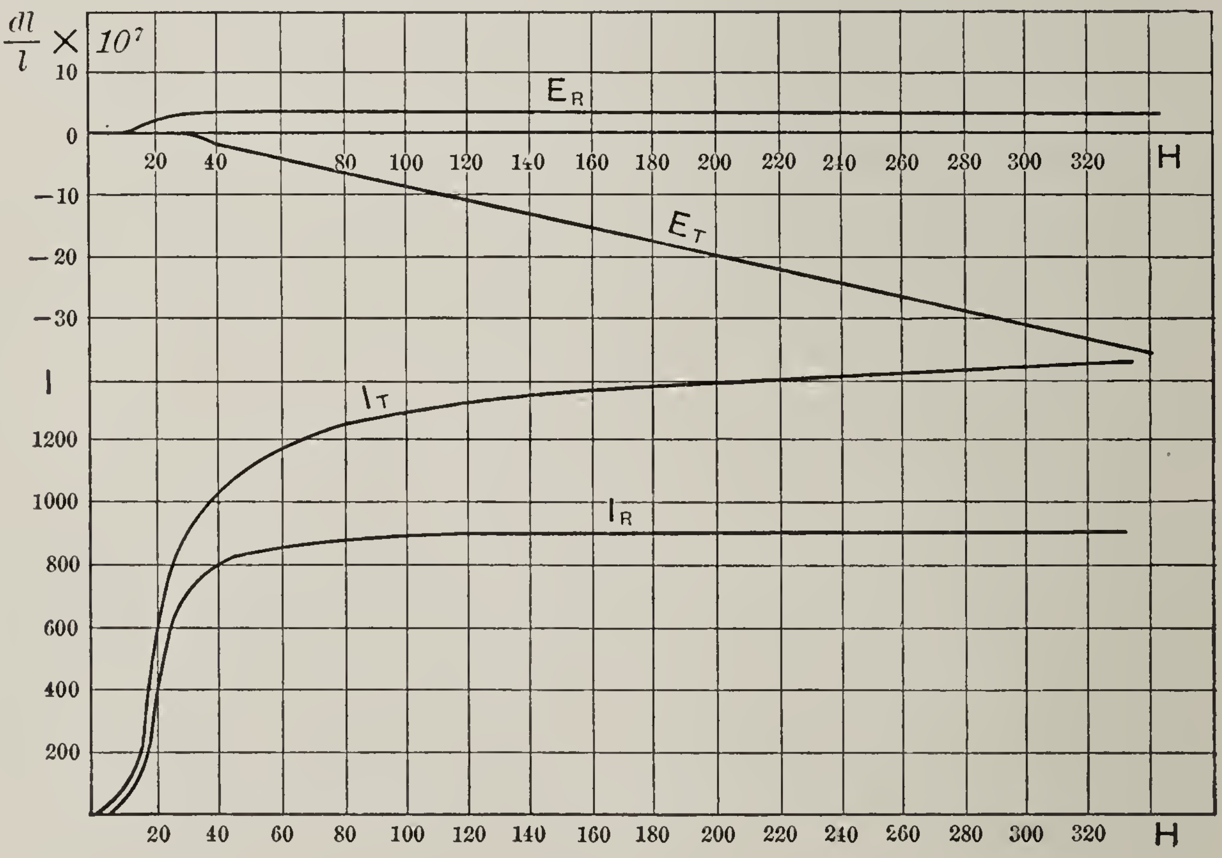

Plate 1.

Natural Piano Wire: Tension 658. (See Tables 1 and 2.)

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[1] Ewing, Magnetic Ind., p. 145.

[2] Bidwell, *loc. cit.*

TABLE I.

Elongation data for piano wire in natural state and under tension of 658 *kg. per square cm.*

$$\frac{dl}{l} \times 10^7 \text{ due to this tension} = 2933.$$

H	E_T	E_R	H	E_T	E_R
.914	.00	.00	59.49	−3.92	3.36
1.83	.00	.00	69.50	−5.04	3.36
2.74	.00	.00	79.50	−6.16	3.36
4.11	.00	.00	94.15	− 7.28	3.36
5.49	.00	.00	104.2	− 8.67	3.36
8.23	.00	.00	116.1	−10.09	3.36
9.14	.00	.00	132.7	−11.78	3.36
13.09	.00	.56	143.1	−13.15	3 36
15.90	.00	1.12	155.5	−15.12	3.36
19.68	.00	1.68	167.3	−16.24	3.36
22.41	.00	2.24	183.0	−17.92	3.36
26.85	.00	2.80	203.5	−20.73	3.36
29.95	−0.56	3.36	219.6	−21.85	3.36
33.15	−0.84	3.36	240.0	−24.66	3.36
38.10	−1.40	3.36	260.6	−26.90	3.36
45.08	−2.52	3.36	278.8	−29.68	3.36
48.72	−3.08	3.36	301.8	−31.39	3.36
53.50	−3.47	3.36	338.5	−35.30	3.36

EFFECTS OF TENSION.

As to the effects of tension, it will be seen that up to the limit of my magnetizing field, the numerical sum of the elongation and contraction is nearly the same for all tests of the same kind of wire regardless of the tension used. But the greater the tension the less will be the elongation and the greater the contraction obtained. Whether we examine the curves as plotted to H or to I, we shall see that after the contraction begins, the length of the wire as compared to its length before magnetization, becomes less as the tension is greater. That is, if we disregard, for the moment, the elongations caused by the tensions used, and consider the wires to all have equal lengths when magnetization begins, then we may say that on the parts of the curves which we are considering, for equal fields or for equal inductions, the greater the tension the shorter the wire. For example, if we examine the total elongation curves, as

Table II.

Magnetization data for wire in Table I.

H	B	μ	I_T	I_R
1.83	98	53.6	7.7	.0
2.75	169	61.5	13.2	.75
6.40	459	71.7	36.1	6.4
8.09	707	87.4	55.6	14.1
10.75	982	91.4	77.3	25.1
12.71	1374	108.1	108.3	36.1
15.50	2394	154.6	189.2	99.2
19.45	5814	299.5	460.5	339.5
21.98	7747	353.0	615.2	488.0
26.65	10167	381.5	807.5	652.5
29.52	11020	374.0	874.5	709.0
32.82	11843	361.2	939.5	751.0
37.65	12578	334.0	999.9	794.0
40.73	13091	322.0	1038.	805.
44.45	13464	303.0	1069.	830.
48.00	13798	287.8	1095.	839.
52.75	14133	268.1	1120.	861.
58.35	14608	250.5	1159.	864.5
66.65	15117	227.0	1199.	876.
76.50	15637	204.5	1238	883.
91.42	16141	177.0	1238	890
101.2	16351	161.5	1294	896
112.5	16663	148.2	1318	898
128.4	16848	131.2	1331	900
139.2	16969	121.9	1338	900
151.0	17271	114.4	1363	902
165.0	17345	105.2	1368	902
178.2	17528	98.4	1381	904
198.3	17598	88.8	1385	904
217.4	17937	82.4	1412	906
238.0	18188	76.4	1428	908
256.2	18206	71.1	1428	908
274.5	18375	66.9	1440	910
297.3	18447	62.2	1445	910
331.8	18582	56.0	1453	913

plotted to H, for soft wire under least and under greatest tension we shall see that, after the curves begin to descend, all points on the curve of greatest tension are about 40 units below those of least tension. But before any change of length due to magnetization began the elongation caused by the tension, expressed in the

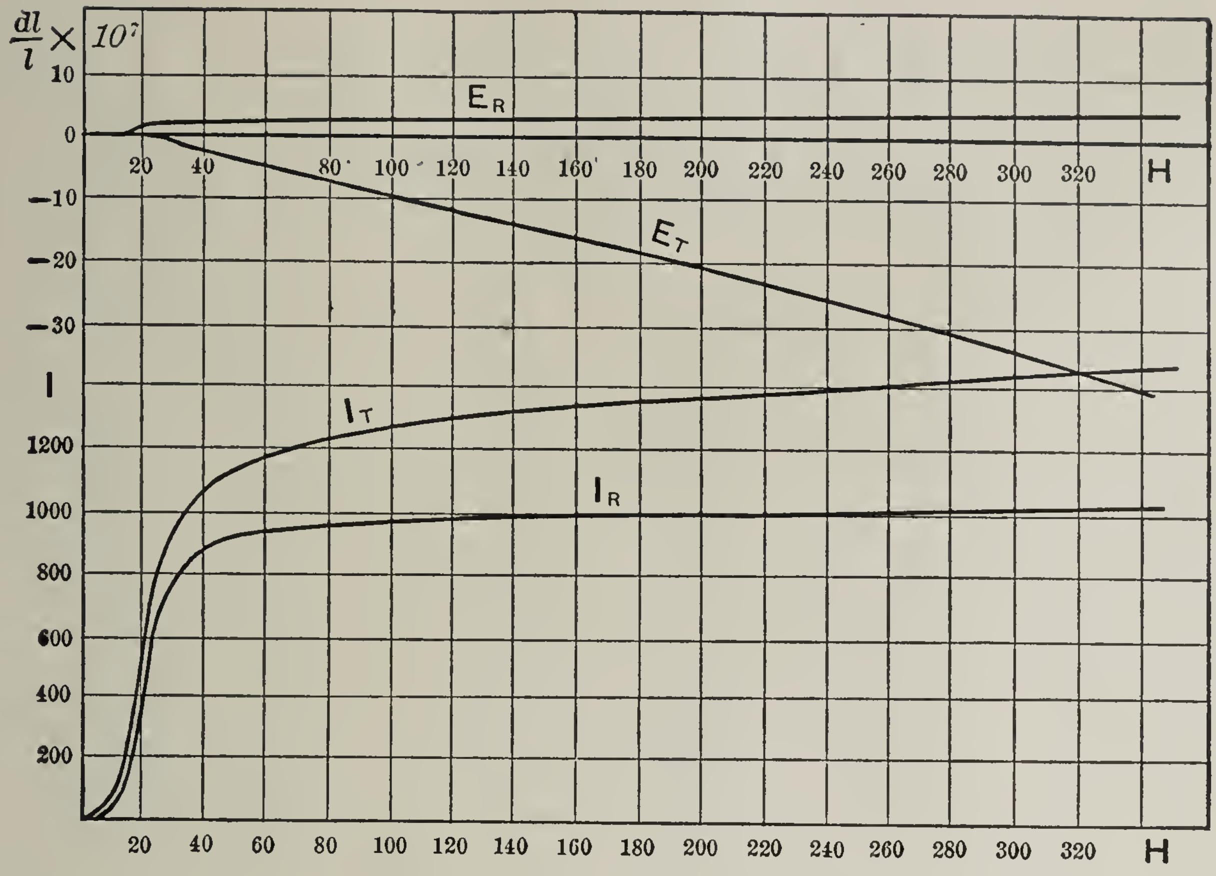

Plate 2.

Natural Piano Wire: Tension 1949.

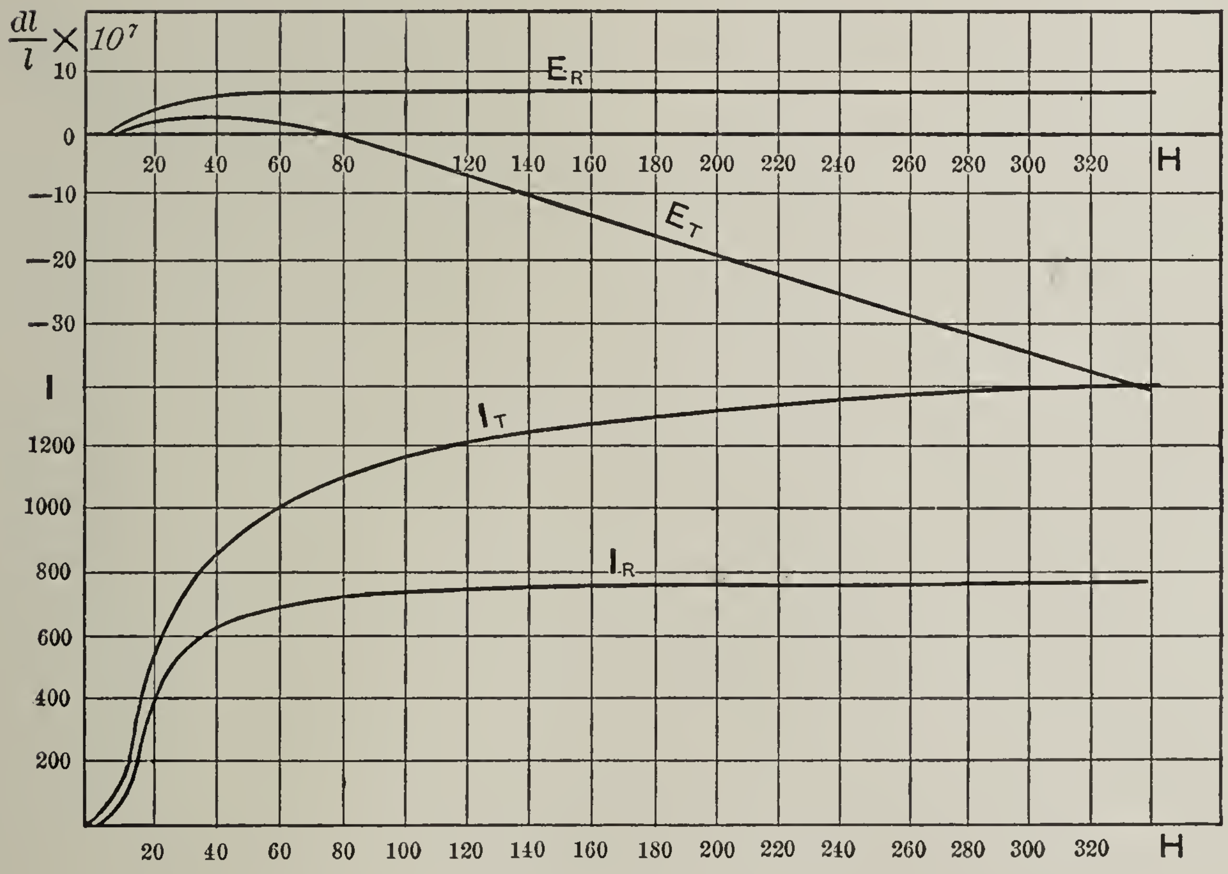

Plate 3.

Annealed Piano Wire: Tension 62.

TABLE VII.

Elongation data for annealed piano wire under tension of 699 *kg. per square cm.*

$\frac{dl}{l} \times 10^7$ due to this tension = 3178.

H	E_T	E_R	H	E_T	E_R
.914	.00	.00	52.18	−0.567	5.39
1.830	.00	.00	58.10	−1.42	5.39
2.74	.00	.00	69.45	−3.42	5.39
3.66	.00	.00	78.22	−4.54	5.39
4.57	.00	.00	93.20	−7.37	5.39
5.49	.00	.00	103.3	−9.06	5.39
8.14	.00	.17	114.7	−10.78	5.39
9.23	.17	.567	131.2	−14.17	5.39
11.95	.567	.850	141.6	−15.88	5.39
12.81	.850	1.42	157.9	−18.70	5·39
15.54	1.70	2.27	166.4	−21.00	5.39
19.69	2.55	4.25	182.4	−23.25	5.39
21.99	2.84	4.82	203.2	−27.20	5.39
27.00	2.84	5.11	219.2	−30.61	5.39
29.72	2.27	5.11	237.9	−32.88	5.39
32.90	1.99	5.39	258.2	−36.84	5.39
37.92	1.70	5.39	274.2	−40.26	5.39
40.98	1.13	5.39	304.1	−44.22	5.39
44.38	.567	5.39	340.8	−49.02	5.39
48.00	.00	5.39			

same units used in the curves was 7950 in one case and only 223 in the other case. What probably occurs is a reduction of the original elongation of 7950 units by 40 units, and beyond this all the phenomenon is very nearly as in a case of no tension. Suppose, then, the wire to become less elastic with magnetization, and that the value of Young's modulus has increased by about one-half per cent.[1] when the turning point in the magnetization curve is reached; then the elongation that originally existed will be diminished by one-half per cent., and we have explained the greater contraction resulting from magnetization when a wire is under tension than when it has no tension. This change in the modulus of elasticity will explain the reduced elongations of all other curves in the soft iron series; and a similar explanation will apply to all curves

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[1] Bock, *loc. cit.*

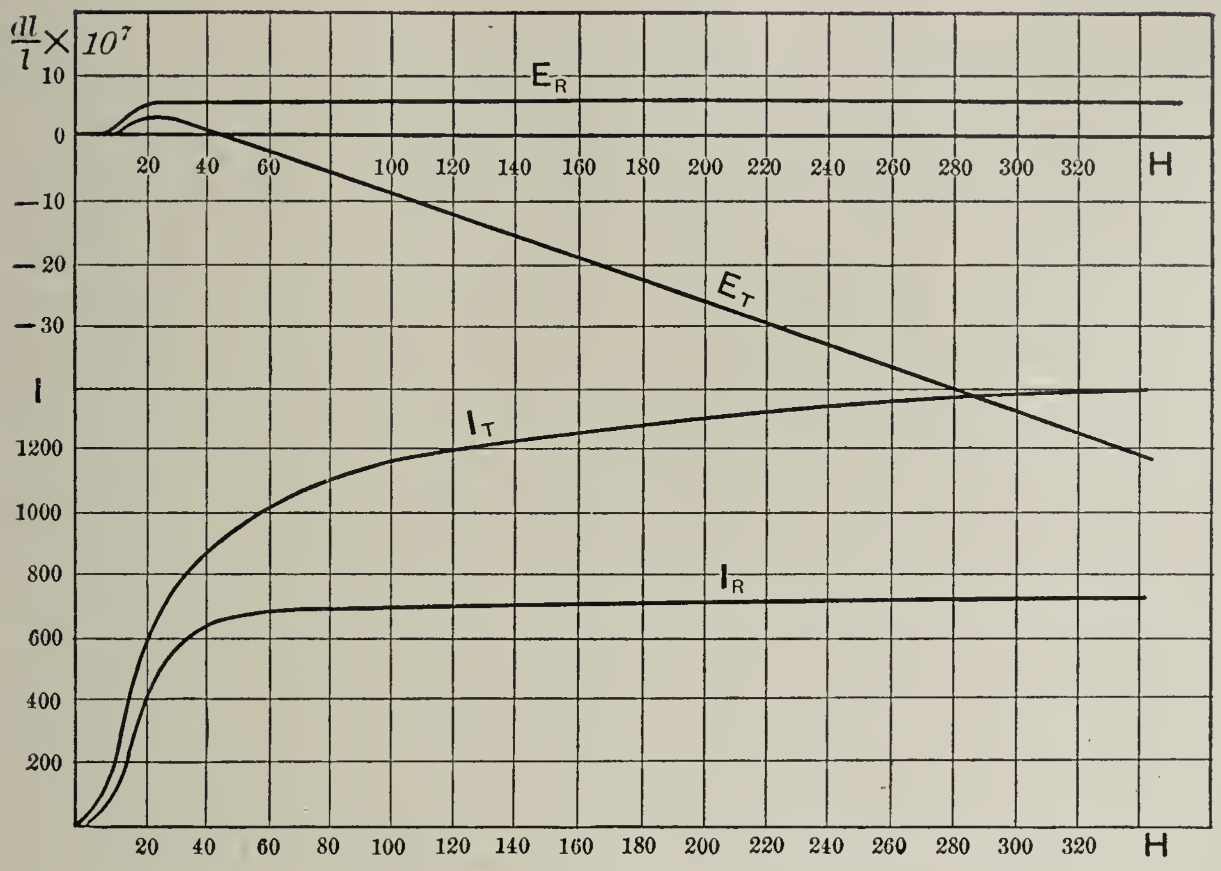

Plate 4.

Annealed Piano Wire: Tension 699. (See Tables 7 and 8.)

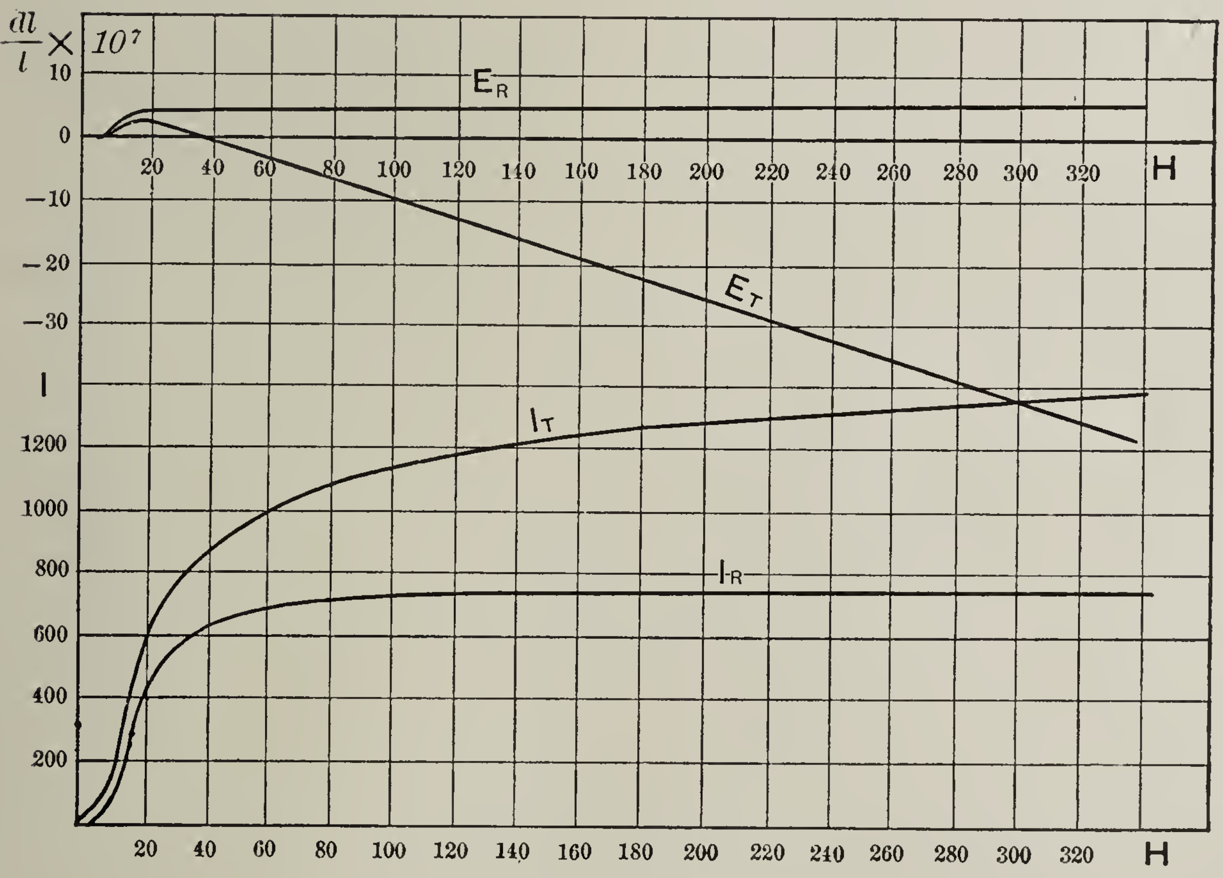

Plate 5.

Annealed Piano Wire: Tension 1949.

Table VIII.

Magnetization data for wire in Table VII.

	B	μ	I_T	I_R
1.83	99	55.	7.7	0
2.65	176	66.4	13.8	2.5
3.66	269	73.5	21.1	3.0
4.67	362	77.6	28.4	3.1
5.49	433	79.0	33.9	6.7
8.09	837	103.6	65.9	25.7
9.15	1024	112.0	80.8	27.7
10.74	1399	130.5	110.6	47.2
12.66	2077	164.5	164.2	88.3
15.18	3581	236.0	284.0	182.0
16.91	4352	258.0	345.0	227.0
19.19	5689	295.6	450.5	324.0
20.80	6306	303.8	500.0	361.8
22.41	6952	310.0	551.0	409.2
30.49	8990	294.0	715.5	553.5
36.80	10327	281.0	819.0	608.8
39.75	10740	271.0	852.5	629.0
43.25	11143	258.0	883.0	651.0
47.18	11577	245.5	917.5	656.0
51.45	11972	233.0	949.0	668.0
56.95	12407	218.3	984.0	679.0
69.01	13369	193.8	1060.	690.
77.00	13707	178.0	1085.	695.
91.45	14482	158.3	1142.	700.
101.6	14592	143.7	1152.	706
112.9	15043	133.0	1190	712
129.0	15389	119.2	1214	712
139.3	15629	112.3	1232	714
151.4	15871	104.6	1251	716
166.2	16066	96.4	1266	717
178.2	16428	92.2	1295	718
198.1	16558	83.6	1301	720
215.4	16915	78.5	1329	722
235.4	17055	72.6	1339	724
251.5	17282	68.7	1356	726
274.5	17375	63.2	1361	726
297.2	17497	58.9	1369	730
327.0	17917	54.8	1399	733

in this investigation, though the change of Young's modulus must be considerably less than one-half per cent. for the annealed piano wire and still less for the natural piano wire.

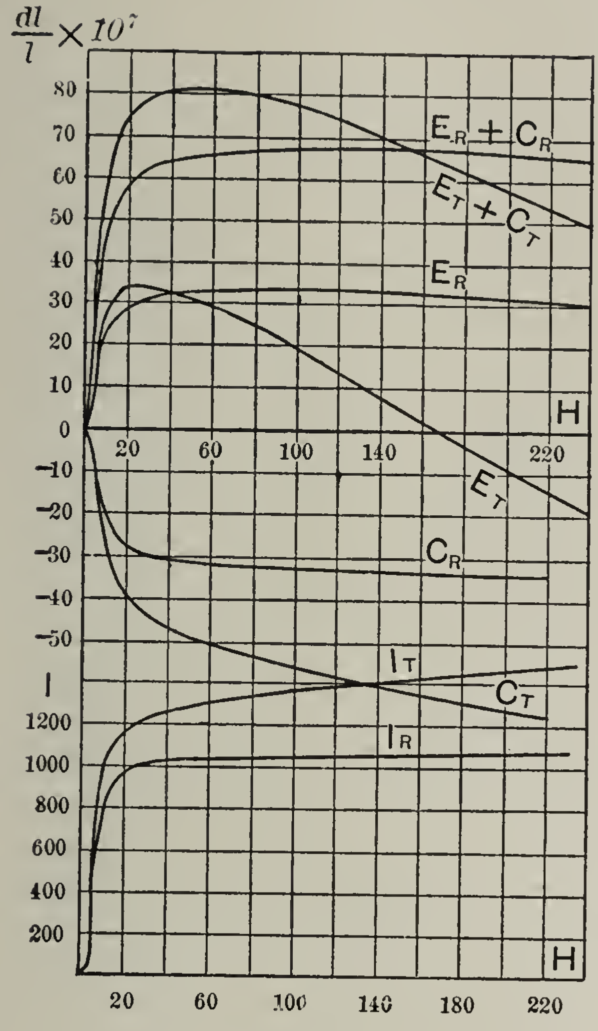

Plate 6.
Soft Iron: Tension 48.3.
(See Tables 11 and 12.)

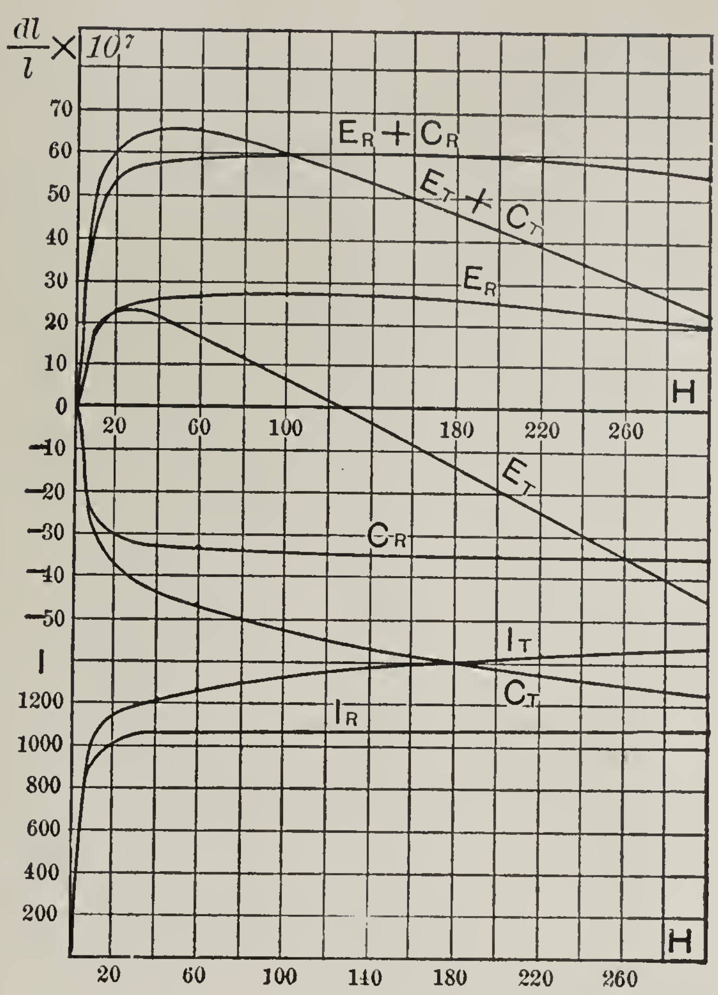

Plate 7.
Soft Iron: Tension 430. (See Tables 13 and 14.)

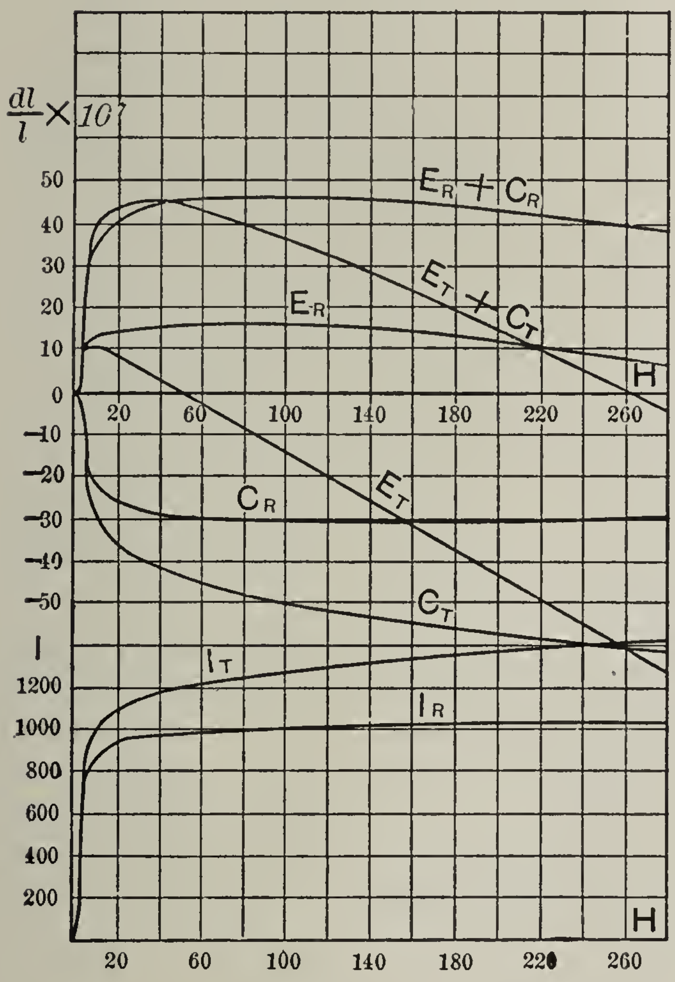

Plate 8.
Soft Iron: Tension 752.
(See Tables 15 and 16.)

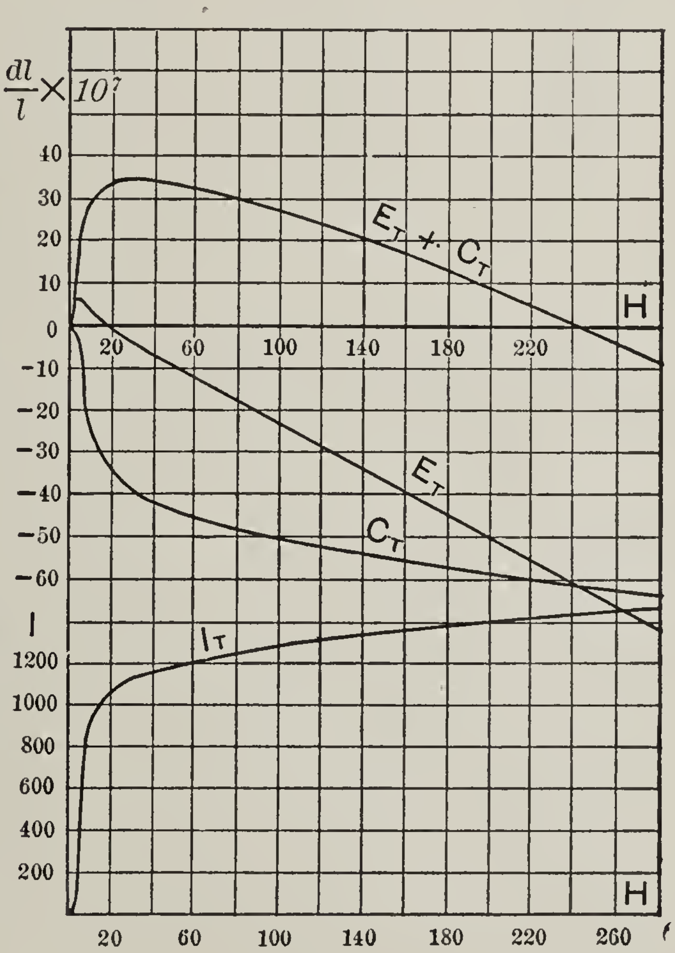

Plate 9.
Soft Iron: Tension 1720.

Table XIII.

Elongation data for soft, annealed iron wire under tension of 430 *kg. per square cm.*

$\frac{dl}{l} \times 10^7$ due to this tension = 1988.

H	E_T	E_R	H	E_T	E_R
.457	0.29	0.28	42.99	20.92	25.92
.915	0.853	0.68	46.48	20.38	26.11
1.51	1.71	1.42	50.68	19.62	26.19
1.82	1.99	1.82	54.82	18.76	26.19
2.42	3.41	2.84	60.08	17.61	26.19
2.88	4.55	4.09	67.00	15.92	26.22
3.43	6.27	5.69	76.50	13.41	26.40
3.93	7.97	7.40	91.95	9.96	26.72
4.62	10.52	9.68	101.4	6.95	26.72
5.26	12.39	11.37	113.2	3.99	26.72
6.12	14.51	13.64	129.5	− 0.57	26.60
7.31	17.06	15.91	139.8	− 3.13	26.60
9.14	19.21	18.18	152.4	− 6.27	26.40
10.40	20.19	19.33	163.7	− 9.68	25.60
12.21	21.32	20.44	178.9	−13.93	25.01
14.41	21.71	21.18	198.6	−19.21	24.42
17.54	22.78	22.77	228.5	−26.16	24.15
22.18	23.20	23.78	246.6	−31.00	23.02
25.32	23.20	24.20	262.5	−36.12	21.00
30.18	22.78	24.78	283.2	−42.99	18.77
33.48	22.20	25.01	308.3	−54.50	15.32
37.39	21.61	25.60			

CONCLUSIONS.

The investigation seems to have established the following laws for magnetization under conditions like those used in my work:

1. Any increase in the magnetic induction tends to lengthen the iron wire. As shown by the curves of elongation plotted to I, the relation between I and elongation is nearly the inverse of the relation between H and I shown in the ordinary induction curve, until well beyond the maximum value of μ, when it curves in an opposite direction on account of the field effect.

2. The magnetizing field tends to shorten the wire and the shortening due to this cause apparently has no limit. Up to a field of two or three hundred the shortening seems to be directly proportional to the field strength and then it seems to begin to approach asymptotically some limiting value.

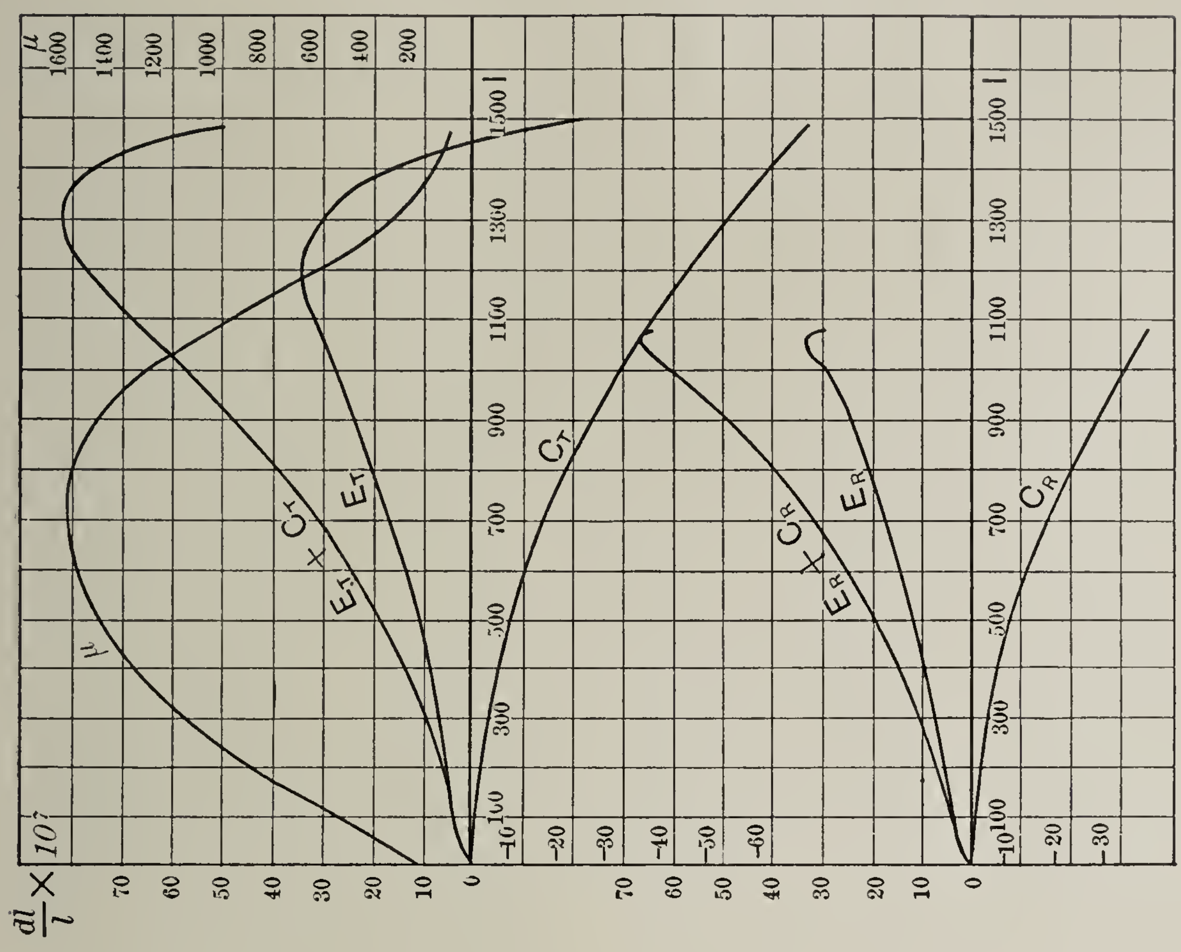

Plate 10.

Soft Iron: Tension 48.3. (See Tables 11 and 12.)

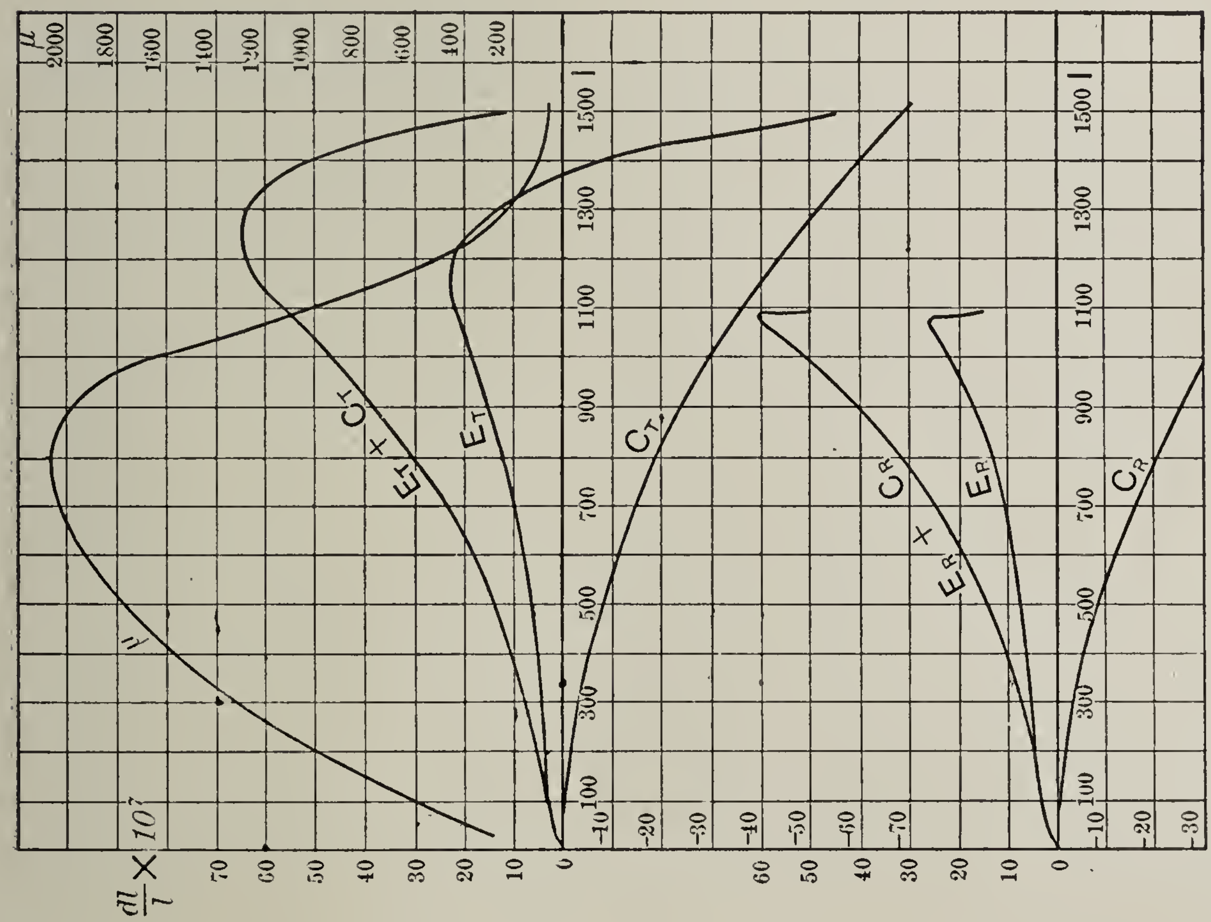

Plate 11.

Soft Iron: Tension 430. (See Tables 13 and 14.)

Table XIV.

Magnetization data for wire in Table XIII.

H	B	μ	I_T	I_R	C_T	C_R
1.46	464	318	36.8	8.09	.04	.0
1.92	752	392	59.7	23.5	.10	.02
2.29	1496	654	119.0	65.3	.42	.13
2.75	3235	1178	255.2	182.0	1.97	.98
3.39	5658	1672	442.5	347.5	6.00	3.57
4.58	9405	2060	748.5	634.5	16.6	11.9
5.22	10695	2060	852.5	733.8	21.4	15.9
6.08	11616	1910	924.2	820.0	25.3	19.9
7.32	12487	1710	994	880.0	29.3	22.9
9.04	12909	1430	1025	915.0	31.3	24.8
10.29	13270	1290	1055	937.5	33.1	26.0
12.09	13572	1120	1079	976	34.6	28.2
14.24	13864	973	1102	987.5	36.1	28.9
17.20	14197	826	1128	1008	37.8	30.1
21.60	14532	673	1154	1026	39.7	31.1
24.63	14775	600	1173	1042	41.0	32.1
29.28	14959	511	1187	1053	42.0	32.8
32.28	15082	468	1198	1053	42.7	32.8
35.81	15296	427	1213	1053	43.9	32.8
38.60	15399	399	1221	1053	44.5	32.8
41.00	15441	377	1225	1055	44.8	32.9
45.00	15605	347	1237	1056	45.8	33.0
52.63	15793	300	1252	1058	46.9	33 1
58.12	15938	274	1263	1058	47.7	33.1
68.60	16229	236	1285	1058	49.5	33.1
78.25	16408	210	1298	1059	50.6	33.1
93.15	16733	179	1324	1070	52.6	33.9
102.9	16903	165	1336	1070	53.6	33.9
114.3	17064	149	1348	1070	54.6	33.9
130.6	17411	133	1374	1074	56.8	34.2
140.0	17462	123	1380	1076	57.3	34.3
154.3	17674	115	1393	1076	58.5	34.3
166.5	17907	107.5	1411	1076	60.3	34.3
183.1	17983	98.0	1416	1078	60.7	34.4
203.8	18244	89.0	1436	1078	62.5	34.4
226.3	18366	81.3	1443	1078	63.3	34.4
249.2	18429	73.9	1446	1080	63.7	34.5
270.0	18790	69.5	1473	1080	66.3	34.5
297.5	18947	63.6	1483	1081	67.4	34.5
341.6	19422	56.8	1516	1092	70.8	35.3

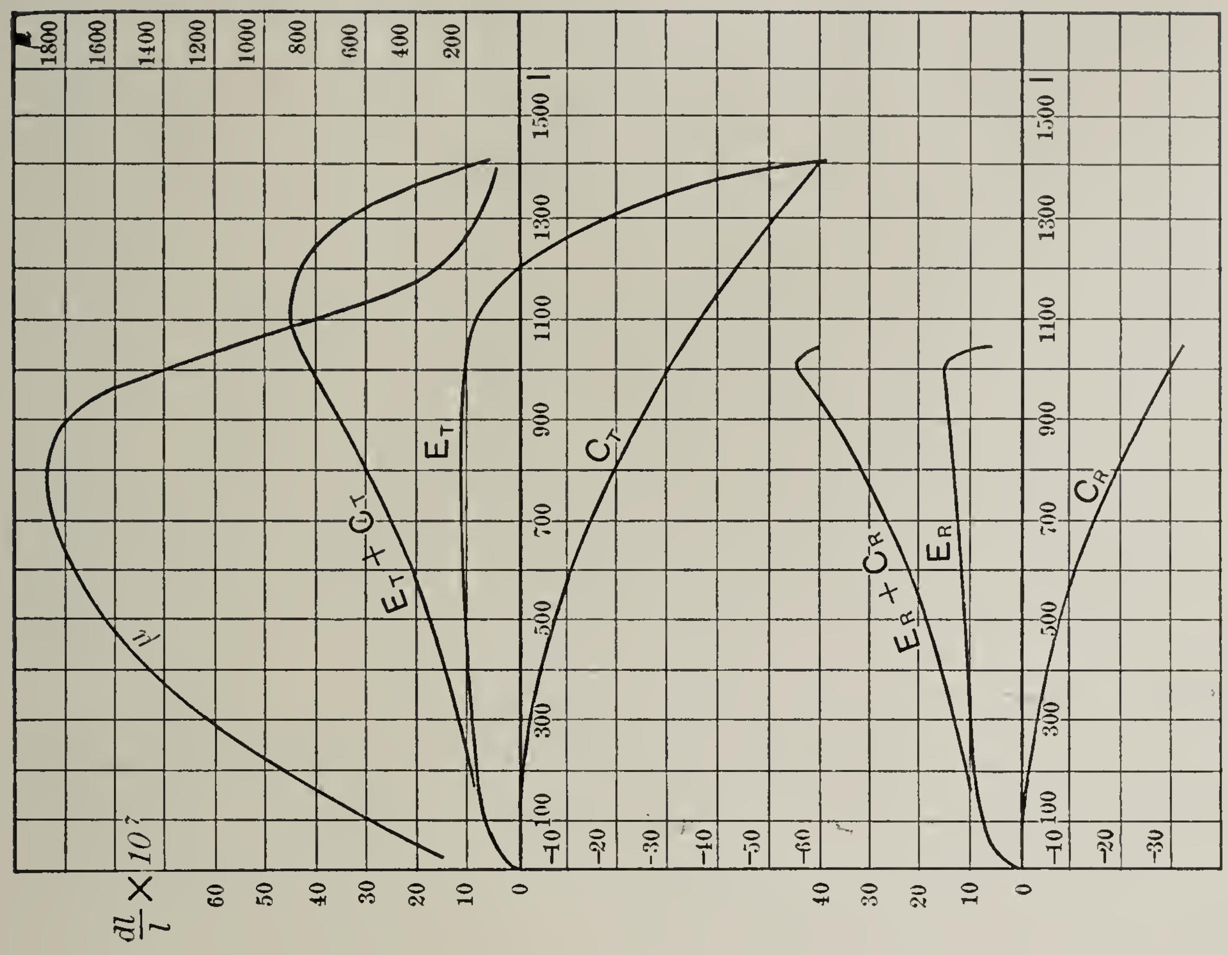

Plate 12.

Soft Iron: Tension 752.

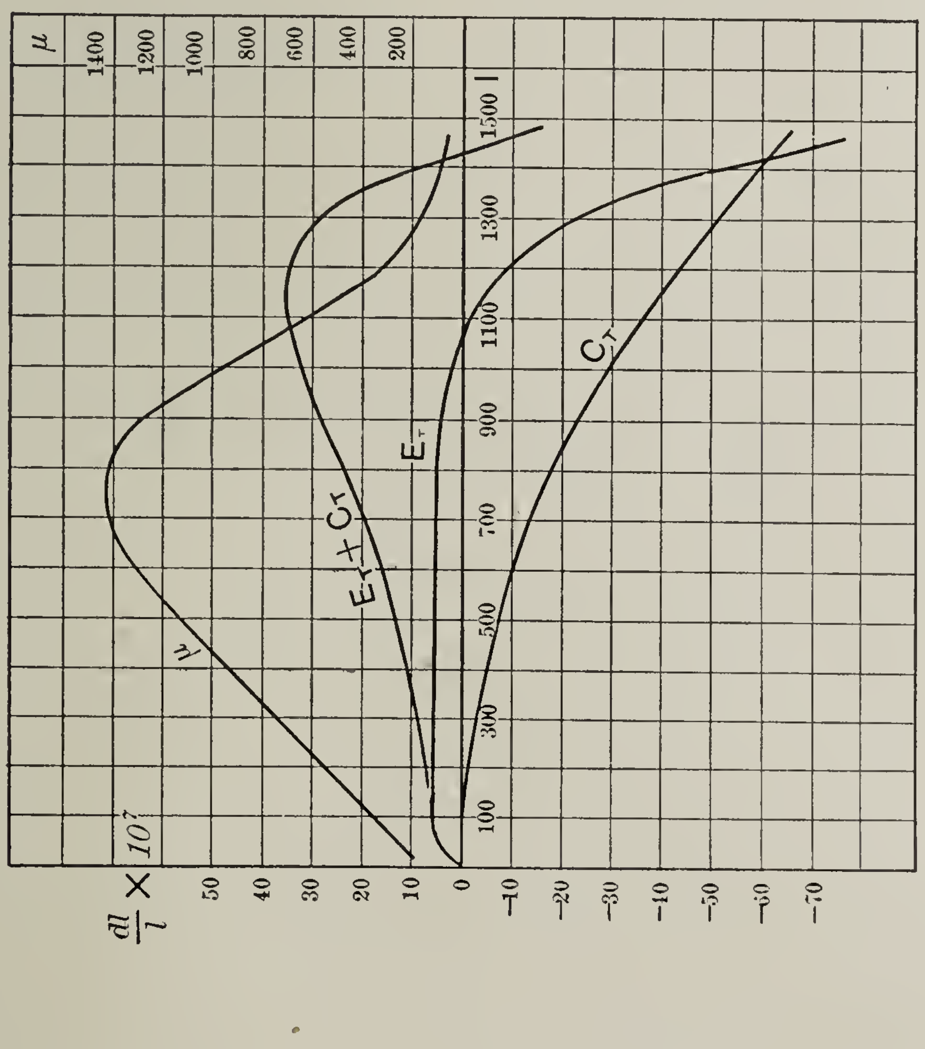

Plate 13.

Soft Iron: Tension 1720. (See Tables 17 and 18.)

3. The elasticity changes with the induction, the modulus being in some cases one-half per cent. greater at the highest magnetization than when the magnetization began. But the law of the change is unknown further than that the elasticity changes only as the induction changes.

In closing, I desire to acknowledge my indebtedness to Professor Rowland, not only for suggesting this investigation to me, but also for help and kind consideration at all stages of the work.

I am also greatly indebted to Dr. Ames, to Dr. Duncan and to Mr. H. S. Hering for valuable suggestions on many portions of the experimental work and for a very considerate interest in the whole investigation.

Explanation of the Tables and the Notation used in the Tables and Plates.

$H \equiv$ the magnetizing field.

$E_T \equiv \frac{\text{change of length}}{\text{length}} \times 10^7$ while H is on.

$E_R \equiv \frac{\text{change of length}}{\text{length}} \times 10^7$ after H is removed.

$B \equiv$ total induction or $4\pi I + H$.

$I_T \equiv$ magnetization of specimen with H on.

$I_R \equiv$ magnetization of specimen with H off.

$\mu \equiv$ magnetic permeability or $\frac{B}{H}$.

$C_T \equiv \frac{\text{change of length}}{\text{length}} \times 10^7$ that would be caused by a mechanical pressure $= \frac{B^2}{8\pi}$.

$C \equiv \frac{\text{change of length}}{\text{length}} \times 10^7$ that would result from a mechanical pressure $\frac{(4\pi I_R)^2}{8\pi}$.

If these compressive forces actually exist then the values of E_T and E_R computed from the observed changes are too small by C_T and C_R respectively and :

$E_T + C_T \equiv$ the real $\frac{\text{change of length}}{\text{length}} \times 10^7$ with H on.

$E_R + C_R \equiv$ the real $\frac{\text{change of length}}{\text{length}} \times 10^7$ with H off.

JOHNS HOPKINS UNIVERSITY, June 30, 1897.

THE DISCHARGE OF ELECTRIFIED BODIES BY THE X-RAYS.[1] II.

By Clement D. Child.

Theory.

THE results thus far given can be explained on the hypothesis given by Thomson and Rutherford.[2] It has been shown that the condition of the gas which is produced by the passage of X-rays through it, after lasting for an appreciable length of time, disappears. This condition is also rapidly destroyed by the passage of a current. It was, therefore, assumed that the gas through which the X-rays pass becomes dissociated, that the ions normally recombine after a short period, and that they recombine much more rapidly when a current passes through the gas. Since I shall have occasion to consider the theory somewhat in detail, the following sentences are quoted from Thomson and Rutherford:

"If we regard the gas as an electrolyte, then the passage of a quantity of electricity will destroy c/ε of the conducting particles, where c is the amount of electricity carried, and ε is the amount carried by one particle.

"Let n be the number of conducting particles in a unit volume of the gas, q the rate at which they are produced by the rays, an^2 the rate at which they disappear independently of the passage of the current, i the current through unit area of the gas, l the distance between the electrodes. Then we have

$$\frac{dn}{dt}=q-an^2-\frac{i}{l\varepsilon}$$

so that when the state of the gas is steady

$$0=q-an^2-\frac{i}{l\varepsilon}.$$

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[1] Concluded from the Physical Review, Vol. V., No. 4.

[2] Phil. Mag., 42, 392.

“ If E is the difference of potential between the plates, U the sum of the velocities of the positively and negatively electrified particles when the potential gradient is unity, we have

$$i=\frac{n\varepsilon UE}{l} \text{ or } n=\frac{li}{\varepsilon UE}.\text{”}$$

Substituting this for n in the above equation and solving for i we find

$$i=\frac{-\frac{1}{l\varepsilon}+\sqrt{\frac{4\alpha ql^2}{\varepsilon^2U^2E^2}+\frac{1}{l^2\varepsilon^2}}}{2\frac{\alpha l^2}{\varepsilon^2U^2E^2}}.$$

High Potential.—Now if E is very large $i=q\varepsilon l$ and the current is not determined by the potential nor by U; but is proportional to the rate at which conducting particles are formed. If, therefore, q is proportional to the square root of the density of the surrounding gas, we should have the current in the case of a large potential proportional to the square root of the density, and we should also have the limiting currents proportional to the same thing. This is approximately what was found to be the case.

Low Potential.—If, however, E remains very small while q becomes large, $i=\frac{\varepsilon UE}{l}\sqrt{\frac{q}{\alpha}}$. It is evident that both U, the velocity of the ions for unit potential gradient, and α, the rate at which the ions recombine when there is one per unit volume, may be functions of the density of the gas. As the density is increased we should expect U to decrease and α to increase. Such a change in either would account for the fact that with small potentials i becomes smaller as the density of the gas is increased.

Density for Maximum Discharge.—If now we make some assumption regarding the dependence of U, α, and q upon the density of the gas, it will be possible to find a value of d which will cause i to have a maximum value. It is apparent from the experimental work that q, the rate at which ions are formed, is roughly proportional to the square root of the density of the gas. Let us, therefore, say that $q=k\sqrt{d}$ where d is the density of the gas, and k is a quantity depending upon the intensity of the X-rays. If we assume that

$\frac{a}{U^2}=k'd^{\frac{3}{2}}$ where k' is a constant, it can be shown that $d_m=\frac{E}{2l}\sqrt{\frac{3}{kk'}}$, where d_m is the density that will cause i to be a maximum. I have no reason *a priori* for supposing that $\frac{a}{U^2}$ varies as the 3/2 power of d rather than according to some other power. This is assumed simply because it gives a value for d_m more consistent with the experimental results than any other supposition. It has already been shown experimentally that d_m varies approximately directly as E and inversely as the square root of k. I also plotted curves between i and d assuming that $\frac{a}{U_2}=k'd^{\frac{3}{2}}$ and $q=kd^{\frac{1}{2}}$. These curves agreed fairly well with those found experimentally, the dotted curves in Fig. 6 being the theoretical ones, and the smooth, the experimental.

If the assumption is made that $\frac{a}{U^2}=k'd^2$, then $d_m=C\frac{E^{\frac{5}{4}}}{k^{\frac{5}{2}}}$, where C is a function of l and k. This agrees nearly, but not fully, as well with the experimental results as the other value for d_m. The curves between i and d which we should then have also agree fairly well with the curves found. No other relation between d and $\frac{a}{U^2}$ explains the experimental results so well.

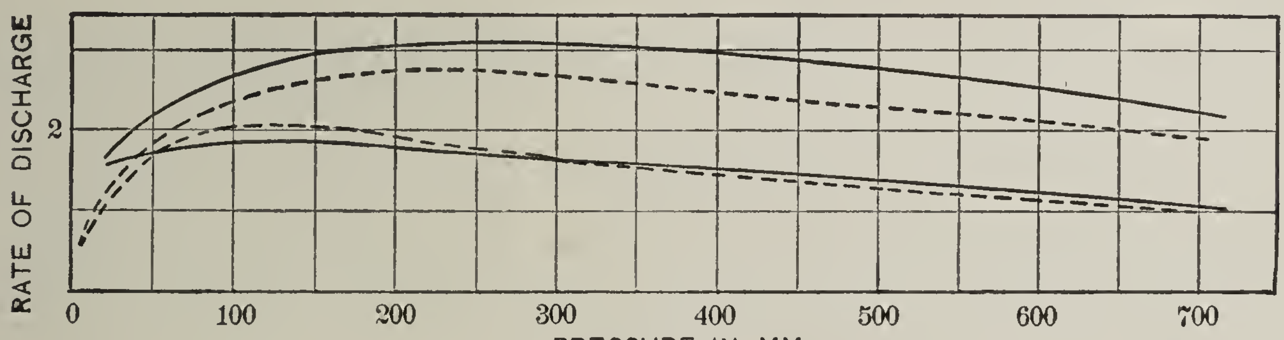

Fig. 6.

Rate of Recombining.—Thomson and Rutherford state that the rate at which the dissociated ions disappear independently of the passage of the current is proportional to the square of the number dissociated. That this is true rather than that they disappear at a rate proportional to the first power, appears from the experimental results here given. If the later assumption be made, it can be shown that d_m will not depend at all on k, that is on the intensity of the rays. But experiment shows very clearly that d_m does depend on the intensity of the rays.

This same assumption can also be tested in another way. It has already been shown that if E is large, the current is proportional to q, the number of ions dissociated, but if E is small the current is proportional to the $\sqrt{q}$. It is most natural to suppose that q is proportional to the intensity of the rays, and thus it follows that a change in the intensity of the rays produces a greater effect on the rate of discharge when E is large than when E is small. This was tested and it was found that such was actually the case. With a difference of potential of 60 volts the fall in potential was 4 volts when the intensity was strong, and 2.1 when it was weak. With a difference of potential of 5 volts the fall was 1 volt when the intensity was strong, and .7 when it was weak. The ratio in the former case was 1.9 and in the latter 1.43. The difference in the two cases corresponds very closely with the difference which the theory calls for. This fact is an important one to bear in mind when one is measuring the intensity of X-rays by the rate of discharge which they produce.

Variation in Number of Discharges.—It is very evident that some more direct way of determining the relation between $\frac{a}{U^2}$ and d is desirable. It seemed possible that by varying the number of discharges per second through the tube, some idea might be obtained concerning this relationship. For this purpose a mercury interrupter, having a device for regulating the number of interruptions per second, was substituted for the magnetic interrupter.

It was first found that when the potential of the charged body was large, the rate of discharge was directly proportional to the number of interruptions up to 12 per second, but there was not this relationship when the potential was small. Curves are plotted in Fig. 7, showing the relation between the interruptions per second and the rate at which the electrified body is discharged in gas at different densities. These were taken with an initial potential of 5 volts.

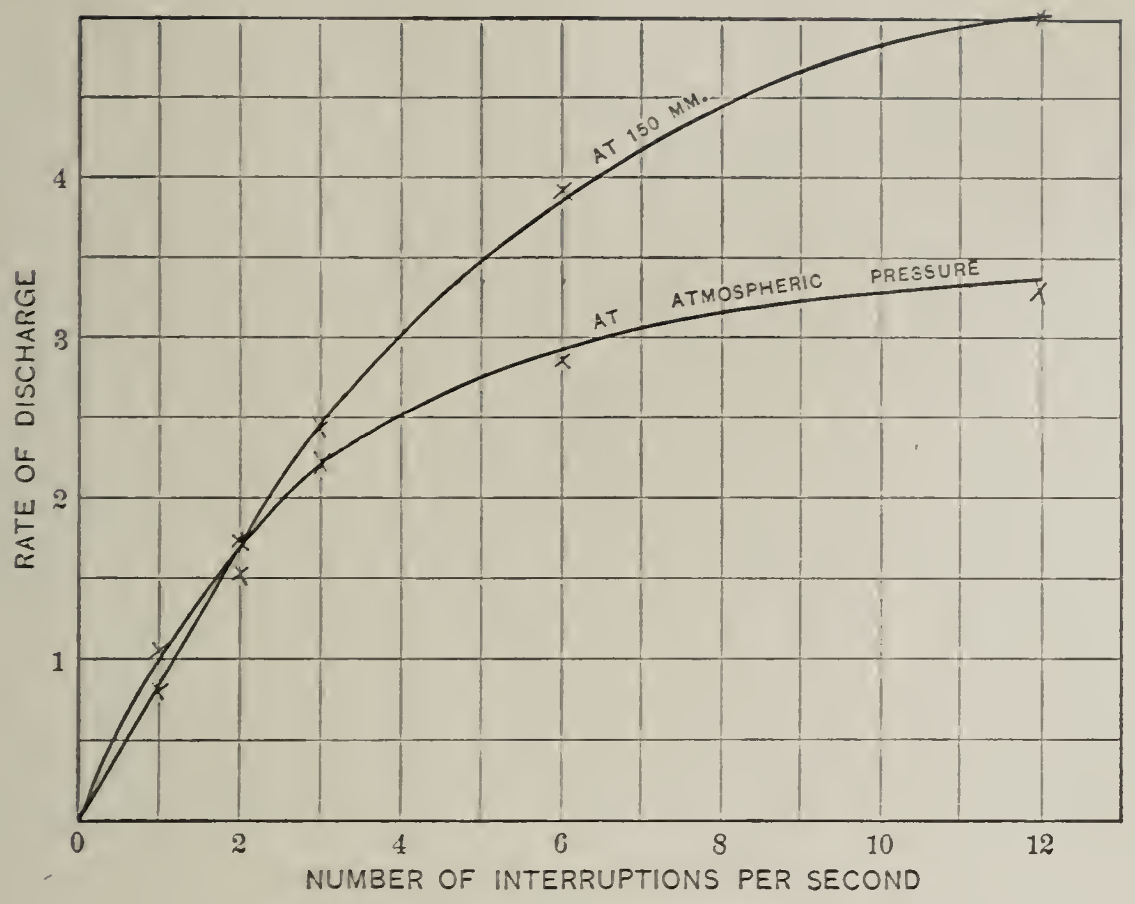

Fig. 7.

It will be noticed that, according to these curves, when a large number of interruptions in the primary circuit takes place, the discharge is more rapid for small densities of the gas than for larger. This is not the case when there are but two or three interruptions per second. It would appear, therefore, that if we should vary the density of the gas, using but few interruptions per second, there would be no maximum current, even though the potential were small. This was found experimentally to be the case.

It is evident that these results are of considerable importance, if a correct explanation for them can be found. In the first place, it is necessary to explain the fact that the rate of discharge is proportional to the number of interruptions per second when the potential is large, and is not proportional when it is small. It has been estimated that the ionized condition of the gas continues for about $\frac{1}{10}$ of a second. It is evident, moreover, that the time will be shorter if a large discharge takes place, and the normal rate of reassociation is increased by the passage of the current through the gas.

If, then, the discharge due to one flash of the rays from the tube is finished before another one occurs, the total discharge will be directly proportional to the number of flashes which are sent out. If, however, one such discharge is not completed before another occurs, such a direct proportion does not hold. For the rate at which the ions normally recombine is assumed to be proportional to the square of the total number present. When few ions are left from a given flash of the rays, their rate of recombining is small, as compared with the number still present, and the current which they carry before they completely recombine will be appreciable.

If, however, a new supply of ions is suddenly added, the rate of recombining is much larger, the ions left over from the first supply are quickly recombined and the amount of electricity carried by them is inappreciable. In this case the total amount of electricity carried by any one supply of ions is less than it would have been if each supply had acted independently. This would explain the fact that the rate of discharge is no longer proportional to the number of interruptions in the coil, when this number is large.

If this be true, then it appears from the curves in Fig. 6 that at atmospheric pressure the discharges begin to interfere with each other when there are 2 per sec., but at a pressure of 150 mm. they do not interfere with each other until there are about 6 per sec. In other words, they more quickly recombine at lower pressures.

This may be caused either by the rate at which they normally recombine being greater at the lower pressure, or by an increase in the rate at which they recombine due to the current. However, if it were due to the fact that they normally recombine more rapidly, the ions would not have as great a chance to carry electricity, and the amount of electricity discharged would therefore be less at the lower pressure. This is not the case.

We are, therefore, driven to the other explanation, namely, that the current causes the ions to recombine more rapidly when the pressure is low. If the velocity of the ions were greater at the lower pressure, then the ions would carry more current and would also be recombined more rapidly. This explanation would, therefore, account for both of the observed facts.

Modification of Theory—Action Discontinuous.—Let us, therefore, return to the mathematical part of the theory. Thomson and Rutherford assumed that the number of dissociated ions in the gas remained constant, being replenished by the action of the X-rays at a uniform rate equal to the rate at which they recombined. It would appear from my last set of curves that when the potential is large, the action of the ions is not continuous, but that the gas resumes its normal condition after each discharge of the tube. The assumption in this case is not only not in accordance with the facts, it is not even an approximation. It will, therefore, be of interest to work the theory through on a more correct assumption in this particular.

A graphical representation of what takes place can be given by curves similar to those in Fig. 8. The ordinates correspond to the number of ions present, the abscissæ to the time. Since the rate of discharge is proportional to the number of ions present, this rate at any instant is also proportional to the corresponding ordinate. The total discharge is proportional to the area of all the curves.

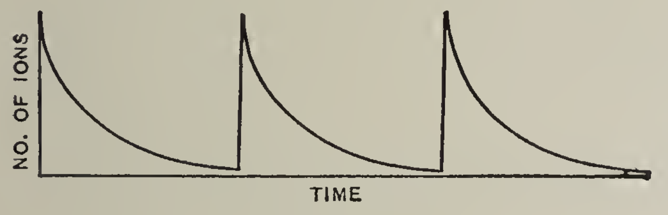

Fig. 8.

First let us find the quantity of electricity discharged by one supply of dissociated ions. Let b equal the rate at which the ions recombine due to the action of the current. Then $\frac{dn}{dt}=-an^2-bn.$

Solving this differential equation we find that $n=\frac{n_0 b\varepsilon^{-bt}}{an_0(1-\varepsilon^{-bt})+b}$ where n_0 is the number of the ions when $t=0$. This would be the number due to one flash of the X-rays. Now $i=\frac{\varepsilon UE}{l}n$ and if Q represents the total quantity of the electricity discharged by this one supply of ions,

$$Q=\frac{\varepsilon UE}{l}\int_0^\infty ndt=\frac{\varepsilon UEn_0 b}{l}\int_0^\infty \frac{\varepsilon^{-bt}}{an_0(1-\varepsilon^{-bt})+b}\,dt=\frac{\varepsilon UE}{la}\log\frac{an_0+b}{b}$$

$$\text{But } bn=\frac{i}{l\varepsilon}\text{and } i=\frac{\varepsilon UE}{l}n \therefore b=\frac{UE}{l^2}\text{ and } Q=\frac{\varepsilon UE}{la}\log\frac{an_0+\frac{UE}{l^2}}{\frac{UE}{l^2}}$$

If this be evaluated for a large value of E, we find that $Q=n_0\varepsilon l$. If there are m discharges per sec. and each one finishes its action before another one begins, the total discharge per sec. is $nQ = mn_0 \varepsilon l$. This agrees identically with the result given by Thomson and Rutherford when we remember that q must be considered equal to mn_0. This also agrees with the fact that in this case the rate of discharge is directly proportional to the number of interruptions per second.

If now we go back to the expression for Q and evaluate it for a small value of E, we find that

$$Q = \frac{\varepsilon l n_0 \frac{UE}{l^2}}{an_0 + \frac{UE}{l^2}}$$

and if $\frac{UE}{l^2}$ is very small as compared with an_0

$$Q = \frac{\varepsilon UE}{al} \text{ and } Qm = \frac{m\varepsilon UE}{al}.$$

Thomson and Rutherford found $i = \frac{\varepsilon UE}{l}\sqrt{\frac{q}{a}}$ when E was small.

The value for Qm would seem to be incorrect in that it is independent of n_0. It must be remembered, however, that this does not hold except when n is very large and E very small, and the action is entirely discontinuous. There would be the greatest difficulty in detecting the rate of discharge under these circumstances. It can not be claimed that any experimental data found by myself were taken under circumstances which would satisfy these conditions.

Action Nearly Discontinuous.—Let us next consider the case when the action is nearly but not quite discontinuous. In this case the ions would be nearly recombined before a second discharge occurred. The few remaining uncombined when the second group of ions was supplied would recombine so quickly, due the presence of additional ions, that their presence could be neglected. In that case

$$Q = \frac{\varepsilon UE}{l} \int_0^{t_1} n dt$$

where t_1 is of such a value that ε^{-bt_1} is nearly equal to zero. If this be integrated and evaluated when E is small, we find

$$mQ = m\frac{\varepsilon l n_0 \frac{UE}{l^2}}{a n_0 + \frac{UE}{l^2}}\left(1 - \varepsilon^{-\frac{UE}{l^2 m}}\right), \text{ since } t_1 = \frac{1}{m}.$$

It is apparent that this quantity is not proportional to m because of the term $\varepsilon^{-\frac{UE}{l^2 m}}$. This term will become larger as U becomes smaller, and since there is greater deviation from the law of strict proportionality in the case of discharge in a gas of greater density, one is led to the conclusion that it becomes smaller as the density of the gas becomes greater.

In the case that $\left(1 - \varepsilon^{-\frac{UE}{l^2 m}}\right)$ differs appreciably from 1, it is evident that this factor will cause Q to increase as U increases. These two conclusions are the same that we have derived by more general consideration of the curves in Fig. 7.

Action Nearly Continuous.—It would be desirable to consider the case when the action of the ions is far from being discontinuous and is not completely continuous. This can be done by making one or two assumptions. The value found for i in this case when E is small is practically the same as that found by Thomson and Rutherford. But it is doubtful whether the theoretical work is at present of any value. It is possible to have a theory with more assumptions than facts, and in this case it certainly would be well to test those assumptions made concerning the rate of normal reassociation and the rate due to the action of the current, by some more direct means than has yet been employed, before a more elaborate theory is constructed.

It is thus evident that I am leaving this subject in a very incomplete condition. Both the fundamental assumptions might be tested by more direct methods and the consequences could be worked out more in detail. However, circumstances necessitate a postponement of further work by myself on this subject for the present. It, therefore, seems better to publish what has already been done and trust that the work may eventually be more fully completed.

Physical Laboratory of Cornell University.

MINOR CONTRIBUTIONS.

An Experimental Research on Gravitational Permeability.

By Louis W. Austin and Charles B. Thwing.

THERE are certain suppositions ordinarily accepted as to the physical properties of gravitation. In general, it is supposed that its intensity depends only upon the amount of matter in, and the geometrical configuration of the attracting masses and that it is independent

I. Of the physical conditions, *i. e.*, solid, liquid, gaseous, amorphous or crystalline.

II. Of the chemical combinations in which the masses happen to exist.

III. Of the temperature of the masses or the medium between them.

IV. It is also supposed that there is no directive character to the field, and

V. That the attraction is quantitatively independent of the intervening medium.

That these suppositions are at least approximately true is very probable, but at the same time the degrees of accuracy of these approximations seem to offer suitable problems for experimental research.

There has been extremely little work done on the question of the variability of gravitation compared with that expended on the determination of the gravitation constant and its attendant problem of the mean density of the earth. Of the above supposition, II.[1] and IV.[2] are, we believe, the only ones which have been investigated directly. The last supposition, No. V., is the subject of investigation in the present paper, which is to be considered only as a preliminary notice of the work, which is still in progress.

That there is nothing comparable to specific inductive capacity or magnetic permeability in gravitation can not be proved by astronomical observations, as here there is practically but one medium involved, the ether. Neither does the fact that no screen appears to affect the attraction of the earth for terrestrial objects prove the supposition. For, since the mass of the earth may be considered as acting as though concentrated at its center of figure, the thickness of any screen which might be introduced between the earth and the attracted body would be in-

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[1] Landolt Zeitschrift f. Phys. Chem., Vol. XII., 1894, p. 1.

[2] A. S. Mackenzie, Physical Review, Vol. II., March, 1895, p. 321.

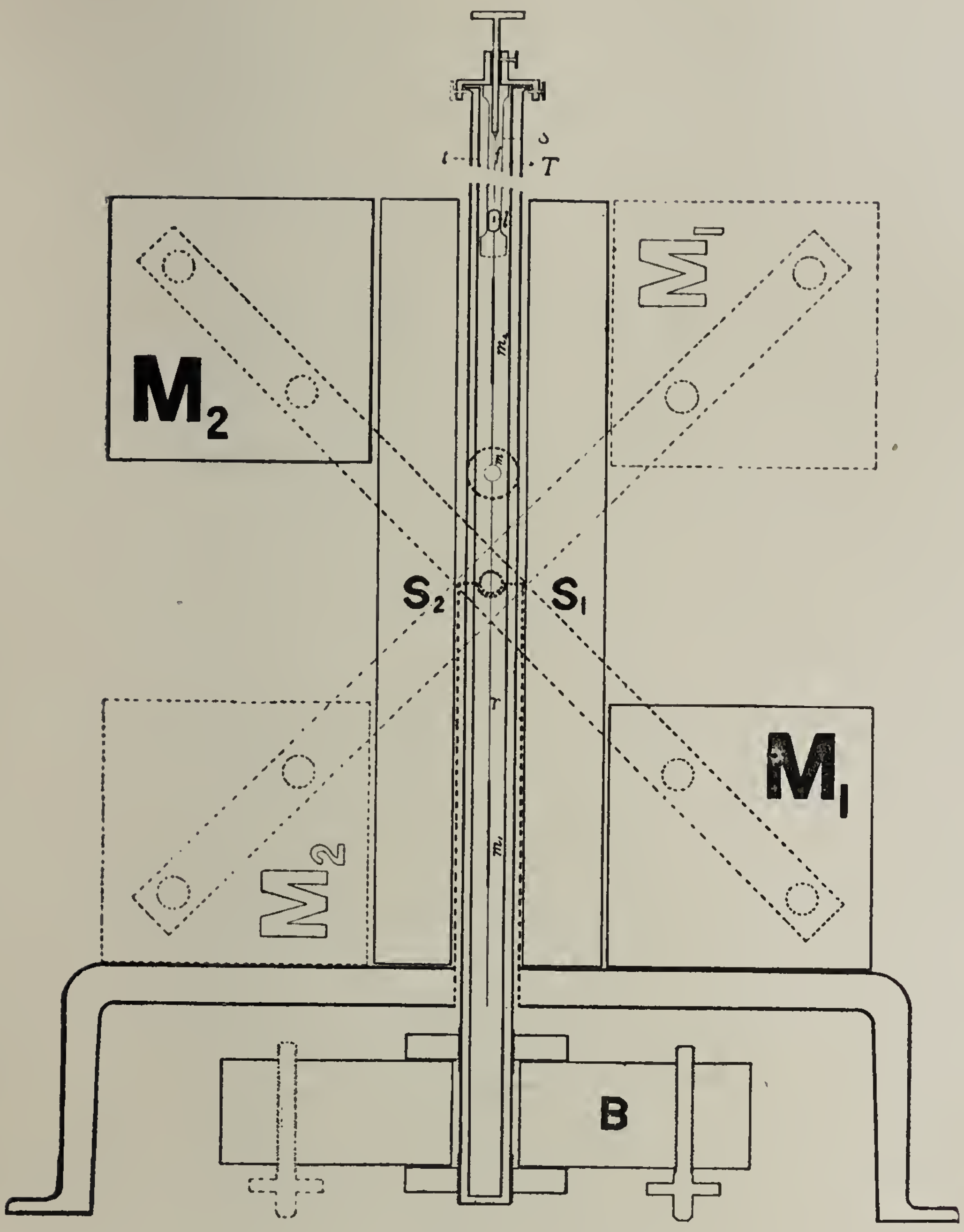

Fig. 1.

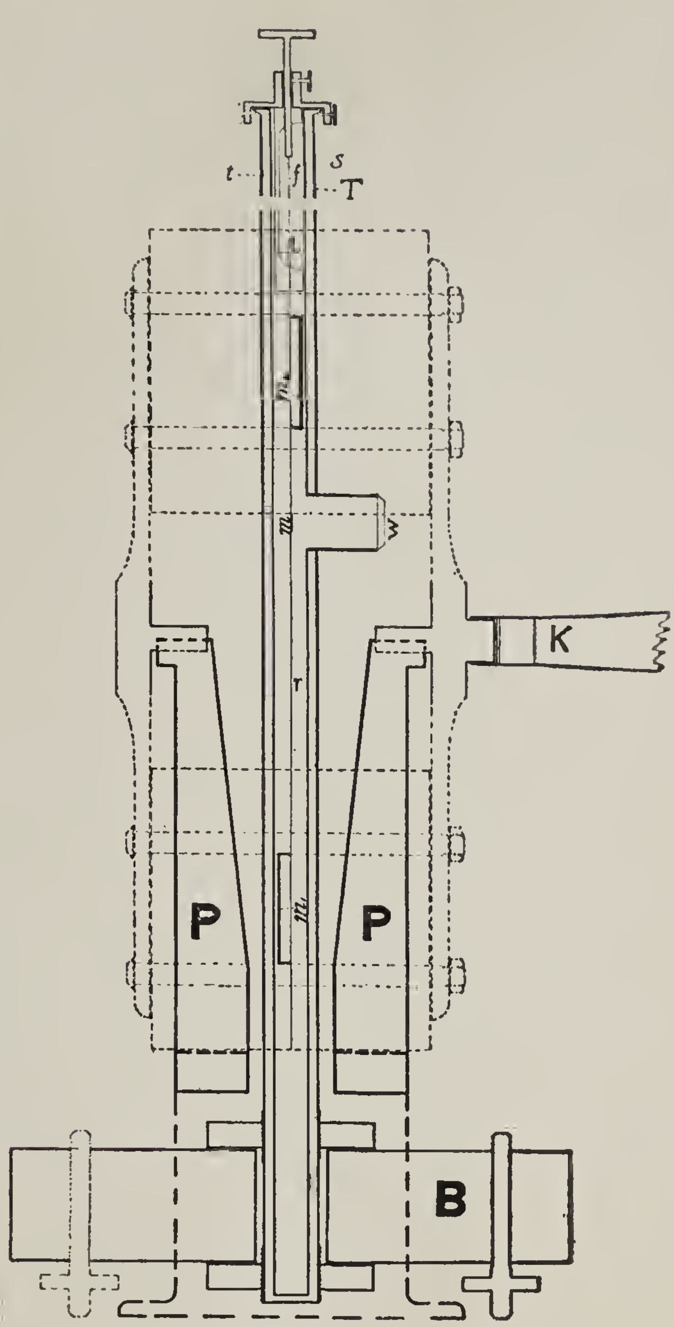

Fig. 2.

finitesimal compared with the total distance between the two masses, and any disturbance in the earth's attraction would be too slight to be observed. Then, too, as has been noted by Professor Poynting,[1] the earth's spherical form might render observations inconclusive, for, just as a series of concentric electrified dielectric shells would have the same external field, whatever their dielectric constant, so the earth might have the same external field whatever the influence that its material might have on the transmission of the gravitational force. The fact, however, that nothing corresponding to the tangential law of refraction of electrical lines of force has been observed in gravitation shows that the supposition is at least approximately true. Again, the fact that the constant of gravitation as derived from cylinders in the work of Wilsing,[2] and,

[1] J. H. Poynting, The Mean Density of the Earth, p. 7; Charles Griffin & Co., London, 1894.

[2] J. Wilsing, Publicationen des Astro.-Phys. Observ. zu Potsdam, No. 22, VI. Band, II. Stück, 1885, p. 35; also No. 23, VI., III. Stuck, 1889, p. 133.

later of Boys,[1] is nearly the same as that derived from spheres, shows that the variation of gravitational permeability is not large. Small as the influence of the medium must be, it seems, nevertheless, worth while to look for it.

The Apparatus.

The work is being carried on in a basement room in the Physical Laboratory of the University of Wisconsin, a room especially well fitted for this purpose, as its temperature is remarkably constant, seldom varying more than 0.5° C. in four hours and often remaining constant within 1° or 2° for weeks at a time. In this room a pier of the most substantial kind was built solid of brick, 2 m. square, on a heavy foundation of stone laid in sand 1 m. below the floor of the room and having on top a stone slab 2 m. square and 20 cm. thick.

The gravitation instrument is of the general form devised by Professor Boys,[2] and the main features are shown in Figs. 1 and 2, which are vertical cross sections through the center of the instrument. Two masses of gold wire (m_1 m_2) each 4 cm. in length, 0.08 cm. in diameter and weighing 0.4 g. are attached to opposite sides of a fine tube of glass (r) at a vertical distance apart of 19 cm. and 0.4 cm. from the axis of rotation. These serve as the attracted weights. To the upper end of the glass tube a loop of fine wire (l) is attached, to the top of which the quartz fiber (f) is fastened. The suspended system weighs 0.98 g. The fiber hangs from a rod which passes through a removable cap, as is usual in galvanometer suspensions. To the under side of this cap is soldered a thin flat supporting rod (s), from the bottom of which a pin projects through the loop (l). This pin and loop make a convenient support for the needle while the fiber is being attached, prevent the fiber from being broken when the system is being lowered into the tube of the instrument, prevent undue rotation when the system is first set free, and save it from damage by falling in case the fiber breaks. Readings are taken from a mirror (m) on a millimeter scale 4.5 meters distant.

To guard against magnetic disturbances only "non magnetic," that is weakly magnetic, material was used in the instrument. Electrostatic disturbances are prevented by the metal case, which is connected to earth. The chief disturbances in any very sensitive instrument of this type are due to tremors and to air currents set up in the tube by temperature changes in the room. To obviate the latter difficulty as far as possible, the tube (t), which had a diameter of 1.4 cm., is enclosed in a second tube (T) having a diameter of 2 cm. and the space between filled with a poor conductor of heat, paraffine. This arrangement of outer and inner conducting tubes with a middle non-conducting layer secures good uniformity of temperature in the interior. This is essential since non-uniformity ordinarily produces the vexatious air currents which form the greatest obstacle to accurate measurements in instruments of very delicate suspended systems. The freedom from these disturbances is such, that an open window with a cold winter wind blowing in within two meters of the instrument hardly disturbed it at all, provided the top was made tight with a rubber washer. The double tube just described is mounted upon a marble base provided with leveling screws.

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[1] C. V. Boys, Phil. Trans., 186 A., 1895, p. 1.

[2] C. V. Boys, Nature, Vol. XLI., Dec. 19, 1889, p. 155.

The deflecting masses (M_1 M_2) consist of two decimeter cubes of lead fastened together by two bars of brass, from which project trunnions which rest in bearings at the top of the two upright posts (P_1 P_2). These form part of a heavy frame of cast brass entirely separate from the instrument itself. By means of a wooden rod having a key (k) at its end the weights can be turned instantly from the position shown by the solid lines to that shown by the dotted lines, without in the least jarring the needle. The weights can also be left horizontal or removed entirely when it is desired to do so. For the purpose of testing the screening or intensifying effect, if such there be, of intervening bodies a space of 3.5 cm. was left on each side between the lead cubes (M) and the tube. In these spaces are inserted screens of the substance to be investigated. When liquids are used they are contained in rectangular copper cans. These screens (S_1 S_2) are 3 cm. thick, 10 cm. wide and 29 cm. high. Since they are symmetrically placed in respect to the needle, their combined effect in producing a deflection of the latter is practically zero.

Method of Observation.

On account of the enormous damping, due to the narrowness of the tube in which the needle hangs, it is found impossible to determine the point of rest by the methods of swings. It is therefore necessary to wait after each reversal of the lead weights until the needle has come to rest (about ten minutes), when readings are begun and continued every half minute for five minutes. The work has been done almost entirely at night in order to gain greater freedom from vibration, which has been by far our worst source of disturbance. Under favorable circumstances, however, and especially late at night the steadiness of the needle is remarkable, being quite as great as that of any very sensitive galvanometer. It has often happened that during the whole ten readings which make up a set the greatest variation from the mean has not exceeded 0.1 mm. on the scale.

We found that there were slow and in general steady changes in sensitiveness for many weeks and in some cases months after the needle was put in place. This was most probably due to changes in the needle itself caused by the glass slowly relieving itself from the strain under which it was almost unavoidably put when the gold weights were cemented in place. We have also recently noticed a change in the sensitiveness with the temperature, the sensitiveness being increased when the temperature rose. This also we believe to be due to a bending of the needle caused by the unequal expansion of the glass and gold. To remedy this in future work platinum weights will be used in place of the gold, unless platinum shall prove too magnetic for the purpose. We have endeavored thus far to make the instrument as free as possible from magnetic disturbance, not even using iron tools in making the needle, but all our efforts have only succeeded in producing a needle which, with our present fiber will not be deflected more than 10 cm. on the scale when a strong bar magnet is brought close to the tube of the instrument.

During the course of our work we have used a number of quartz fibers of different degrees of torsional rigidity. One used in the early part of the work gave a deflection of 107 cm. on a scale 4.5 m. distant, with a time of swing of between six and seven minutes, the exact time of swing being extremely difficult to determine on account of the great damping already mentioned, which brought the needle practically to rest after the first complete swing. Most of the observations thus far have been taken with two fibers, which gave deflections of about 26.5 cm. and 36 cm. respectively with times of swing of about 170 sec. and 190 sec. The first of these fibers, strange to say, broke during the night when not in use and when bearing much less than the weight for which it had been tested before being put in place.

There have been several changes in our methods of work since the earlier observations. At first, with the intention of avoiding the frequent disturbance of the instrument by the shifting of the screens, observations were taken for a week without the screens, then for a week with the screens, then for a week without the screens and so on. But, as it was soon found that the effect of changing the screens seemed to be entirely over in twenty-four hours and as the slow change in sensitiveness seemed to be a more probable source of error, in the later work the screens have been shifted every day or every second day.

There are three properties of matter to which we are looking as likely to be connected with an effect on gravitational permeability. First, density on account of its close connection with gravitation itself. Second, specific inductive capacity on account of the great modifications of the ether which must accompany its variations. Third, for the same reason, magnetic permeability. In the first class lead, mercury and zinc have been examined as screens. In the second, water on account of its high specific inductive capacity was extensively experimented upon. Alcohol and glycerine were also examined. In the third class no observations of any value have been made on account of the magnetic qualities of the needle, although iron is perhaps the one substance that we are most anxious to examine.

In the following table is given one of the series of observations with and without the water screens.

Table I.

WATER.

	Without Screens.	With Screens.	Temp.
1		35.97	24.0
2	36.01		24.1
3		36.07	24.5
4	36.00		24.6
5		36.05	24.5
6		36.00	24.5
7	35.98		24.2
8	36.11		25.0
9		36.11	24.8
Av.	36.025	36.040	

The difference, 0.015 cm, is less than 0.05% of the total deflection. The table also shows the admirable constancy of the temperature in the observing room.

Below are given the three sets of half minute observations from which deflection 9 was calculated:

	Pos. I.	Pos. II.	Pos. I.		Deflection.
	55.79	91.84	55.65		
	.82	.82	.62		
	.82	.82	.62		
	.80	.82	.62	55.79	
	.75	.80	.62		91.81...36.11
	.73	.77	.61	55.72	
	.77	.78	.64		
	.78	.80	.61		
	.75	.79	.63		
	.78	.80	.60		
Av.	55.79	91.81	55.62		

Several other series nearly or quite as satisfactory as this given have been taken on water as well as on the other substances before mentioned, though in many of them the slow changes in sensitiveness are more apparent. The results given serve to show how well the instrument behaves under favorable circumstances. No attempt has been made to apply the method of probable error to the determination of the accuracy of the observations, as disturbances in general seem to tend to move the needle much more in the one direction than in the other, which fact removes the errors from the class which can be treated by that method.

Though the exact degree of accuracy of the observations cannot be ascertained, it can be stated conclusively that the error in our final determinations can, in the case of none of the substances experimented upon, with the exception of iron, much exceed 0.2%. We can thus sum up the results of our work thus far as follows: With screens of lead, zinc, mercury, water, alcohol or glycerine occupying one-third of the distance between the attracting masses and the needle, the change produced in the attractive force is certainly less than 0.2% of the total attraction. In the case of the iron screens no certain conclusions can be drawn, as the presence of the iron modified the magnetic field to such an extent that the sensitiveness of the somewhat magnetic needle was almost certainly changed. The change in the gravitational force, if it exists at all, is, we feel sure, less than 1% of the total force.

In the future work which is to be carried on, it is intended to make use of an instrument with a larger tube, so as to reduce the damping and allow the time of swing and relative sensitiveness to be determined with accuracy. With this it is hoped that the questions of the effect of temperature and of the iron screens can be attacked successfully. The work already done will also be repeated, with the hope of reducing the estimated error to less than 0.1%.

PHYSICAL LABORATORY OF THE UNIVERSITY OF WISCONSIN, June, 1897.

ON THE BEST RESISTANCE FOR A SENSITIVE GALVANOMETER.

BY FRANK A. LAWS.

IT is well known that in the practice of most methods of electrical testing, the precision attainable depends on the proper adjustment of the galvanometer resistance to the work in hand, and that in planning new work a solution for the best galvanometer resistance is always made.

It is our purpose to derive a solution under conditions other than those usually imposed.

To render the discussion more complete, a statement of the usual solution of the problem will be given. It is as follows: If a simple circuit, consisting of a battery, a resistance and a galvanometer, be arranged, the deflection of the instrument will be a maximum when the galvanometer resistance is equal to that of the remainder of the circuit. In obtaining this solution two assumptions are made:

First. That the coils have fixed dimensions.

Second. That the ratio of the diameter of the covered to that of the bare wire is the same for all sizes.

That this second assumption is incorrect is shown by the fact that the ratio for a No. 20 B. & S. guage double silk-covered wire of the American Electrical Works is 1.12, while for a No. 40 wire it is 2.77.

In the course of the discussion we shall use the following symbols:

G, the galvanometer constant, field at centre of coil per unit current.
I_G, galvanometer current.
D, deflection of instrument.
V, volume of the coil.
b, linear constant of the coil, $b = k(V)^{\frac{1}{3}}$.
R_G, the galvanometer resistance.
R, the resistance of the circuit external to the galvanometer.
C, the diameter of the covered wire.
B, the diameter of the bare wire.
$y = C/B$.
w, the resistance per unit volume of the wire.
n, the number of turns per unit area.
E, E.M.F. of battery.
k, constants.

We shall assume an indefinitely short needle, and for the present retain the first assumption.

The usual demonstration is based on the equation,

$$G = k\sqrt{R_G}\,.$$

This equation requires correction. It is easily shown that

$$G = 4\pi nb\,(\varphi\cdot s),$$

where $(\varphi\cdot s)$ is a function the shape of the coil. As the same bobbin is to be used in all cases, we have

$$G = k_1 n = \frac{k_1}{C^2} = k_2\frac{\sqrt{w}}{y} = k_3\sqrt{\frac{R_G}{y^2}}\,.$$

To apply this to the typical circuit we have

$$I_G = \frac{E}{R + R_G},\quad D = k_4\, I_G\, G = k_5\,\frac{E\sqrt{\dfrac{R_G}{y^2}}}{R + R_G}\,.$$

Differentiating and solving for a maximum, we have

$$R_G = \cfrac{R}{\cfrac{2}{y^2\cfrac{d\left(\frac{R_G}{y^2}\right)}{dR_G}} - 1}$$

If y be constant, this reduces to

$$R_G = R,$$

the relation usually given.

This general formula was first given by Professor Silas W. Holman, In order to employ it the properties of the wire must be found by winding experimental coils.

The assumption of fixed dimensions will now be discarded and attention given to the galvanometer constants of a family of similar instruments. For coils of rectangular cross-section we have, referring to the diagram :

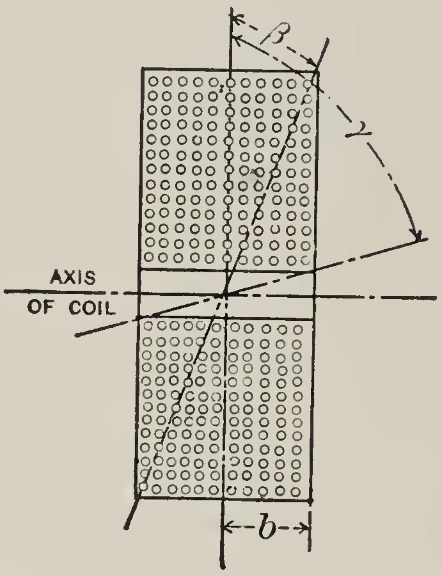

Fig. 1.

$$G = 4\pi nb \log_e \frac{\cot \frac{\beta}{2}}{\cot \frac{\gamma}{2}}.$$

For coils of this shape,

$$\varphi \cdot s = \log_e \frac{\cot \frac{\beta}{2}}{\cot \frac{\gamma}{2}}.$$

For instruments of the same family, β and γ constant, we have :

$$G = k_6 nb = k_6 \frac{b}{yyB^2}$$

$$b = k_7 \left(\frac{R_G}{w}\right)^{1/3},$$

$$y = \frac{k_9}{B^2 w^{1/2}},$$

$$\therefore G = k_{10} \left(\frac{w^{1/6}}{y}\right) R_G^{1/3}.$$

This shows that the sensitiveness of similar galvanometers wound to various resistances with the same size of wire, is proportional to the cube root of the galvanometer resistance. Also for a family of instruments of the same resistance the best size of wire will be that for which $\frac{w^{\frac{1}{6}}}{y}$ is a maximum. By winding experimental coils we obtain data for determining this function, a table of values of which is given below.

WIRE OF AMERICAN ELECTRICAL WORKS.

B. & S. Gauge No.	Single, Silk-covered. $\frac{w^{\frac{1}{6}}}{y}$	Double, Silk-covered. $\frac{w^{\frac{1}{6}}}{y}$	Ratio.
20	.94	.87	1.08
26	1.40	1.12	1.25
30	1.81	1.27	1.42
34	2.21	1.34	1.65
36	2.19	1.33	1.65
38	2.10	1.28	1.64
40	....	1.15	....

The results are plotted in Fig. 2.

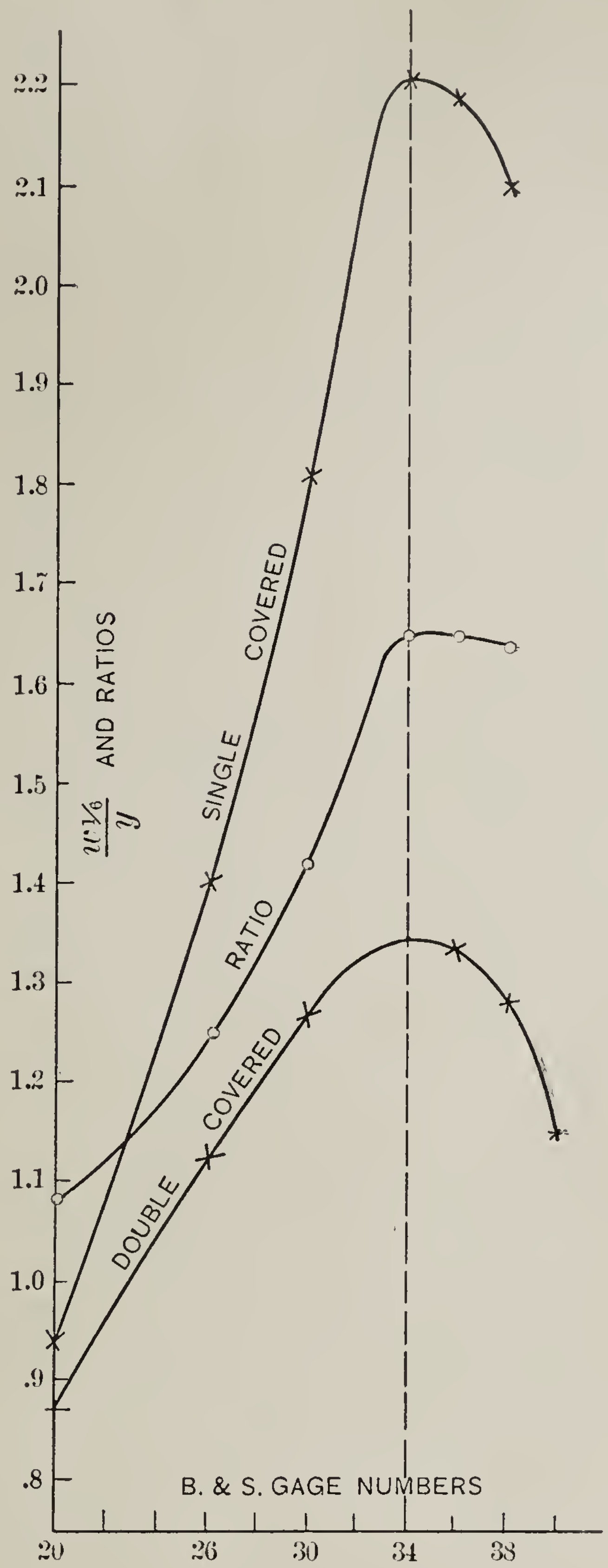

Fig. 2.

Relative galvanometer constants for similar coils wound to the same resistance with various sizes of silk-covered wire.

The best size for both single and double-covered wire is seen to be No. 34. This result of course applies only to the particular make of wire examined.

We see that there may be two instruments of a family having the same resistance which will give equal deflections with the same current.

We are now in position to find the best galvanometer resistance. We have

$$D = k_{10}\left(\frac{w^{\frac{1}{6}}}{y}\right)\frac{R_G^{\frac{1}{3}}}{R + R_G}.$$

Considering for the moment that all the instruments are wound with the same size of wire, we have a maximum value of D when

$$R_G = R/2.$$

Therefore the best galvanometer of the family is one which is wound with a No. 34 wire to a resistance equal to one-half that of the remainder of the circuit.

We will now consider two special examples :—

The coils of two-spool reflecting galvanometers very commonly have a volume of about eight cubic inches. Suppose the resistance external to the instrument to be 10ω and that double-silk-covered wire is to be used ;

then by the ordinary rule the ideal galvanometer resistance is 10ω. In accordance with our first deduction No. 20 wire will give the best winding. R_G will then be 6ω;

$$D = k\, I_G\, G = k_1\, I_G\, nb\,;$$

$$D_{20} = k_1 \frac{E}{16.} 775 \cdot (8.)^{1/3}.$$

For the instrument wound with No. 34 wire we have $R_G = 5.\omega$, corresponding to a volume of coil of .026 cubic inches;

$$D_{34} = k_1 \frac{E}{15} \cdot 7780.\,(.026)^{1/3}\,;$$

$$\frac{D_{34}}{D_{26}} = 1.6 \text{ appox.}$$

The deflection of an instrument similar to the first but designed in accordance with our suggestion, will be 60% greater than that of the instrument wound in accordance with what is called the solution for the best galvanometer resistance.

The coils of eight cubic inches would have a diameter of about 2.6″ and an axial breadth of about 1.5″. The approximate dimensions of the second coil would be diameter .4″, breadth .25″. The weight of wire in the first case would be 30. oz., in the second, .032 oz.

The results we have just obtained are in emphasis of the plea of Professor Boys for small instruments. In this connection we may observe that if we have two similar galvanometers of equal resistance the first having a constant G_1, and wound with wire for which the ratio of the diameter covered to bare wire is y_1, while these quantities for the second instrument are G_2 and y_2, and if the linear dimensions of the first are N times those of the second, then,

$$\frac{G_2}{G_1} = \sqrt{N}\left(\frac{y_1}{y_2}\right)$$

The factor $\frac{y_1}{y_2}$ shows that there is a limit below which it is unprofitable to reduce the size of the instrument, this being the size for which a No. 34 wire is appropriate.

For galvanometers originally wound with wire finer than No. 34, the more sensitive instrument would be the larger.

We will now consider the case of a Wheatstone bridge where the resistances of the arms are small and nearly equal. Let them be of about one ohm each. Such an arrangement would be used in standardizing one-ohm coils or in bolometric work.

The ordinary solution gives one ohm as the best galvanometer resistance. If the coil has a volume of 8 cubic inches, the winding should be of No. 16 wire making $R_G = .88\omega$. We will suppose the E. M. F. of the battery to be 1 volt, its resistance 1 ohm, and that the bridge is out of balance by $\frac{1}{1000}$ ohn; then with due regard to carrying capacity we have

$$I_G = \frac{1}{15000}. \text{ approx.}$$

$$D_{16} = k_1\, 325.\ (8.)^{\frac{1}{3}} \frac{1}{15000}.$$

If wound as here indicated, we should have

$$R_G = .5\omega \quad I_G = \frac{1}{12000}.$$

$$D_{34} = k_1\, 7780.\ (.0026)^{\frac{1}{3}} \frac{1}{12000}.$$

$$\frac{D_{34}}{D_{16}} = 2.04$$

The deflection may be doubled by properly designing the galvanometer. This result is for double-covered wire. If single-covered be used, we should multiply by the ratio factor 1.65, giving

$$\frac{D_{34}}{D_{16}} = 3.36\,;$$

showing that by the use of a proper galvanometer wound with single-covered wire the sensitiveness of the bridge may be increased about 200 per cent.

The gain indicated in the examples is much greater with very low than with high resistance instruments, the former as usually wound departing more widely from the conditions here imposed.

Of course the numerical results indicate chiefly the order of magnitude of the gain for it would be impossible to reduce the size of the instruments as much as is here indicated. Also we have not considered the magnetic system, or the fact that the space at the centre of the coil must be large enough for free movement of the needle and that turns in very close proximity to a finite needle produce less than the effect indicated by the strength of field at the centre of the coil.

Our conclusions are, that if an existing arrangement of apparatus be taken, we can increase its sensitiveness by replacing the galvanometer by a similar instrument wound with wire of a particular size to a resistance equal to one-half of that of the remainder of the circuit. This best size of wire is that for which $\frac{w^{\frac{1}{6}}}{y}$ is a maximum.

For low resistance instruments the reduction in size renders magnetic shielding possible without undue clumsiness.

Rogers Laboratory of Physics, Massachusetts Institute of Technology, July, 1897.

A Lecture Experiment to show the Influence of Ultra-Violet Light on the Spark Discharge.

By Ernest Merritt.

THE discovery of the influence of the Röntgen rays and Becquerel rays in accelerating the discharge of electrified bodies naturally attracts attention to other phenomena of a similar character. Most prominent among these is the effect of ultra-violet light in discharging negatively charged metals. This effect being also an important one on its own account, I have tried during the past year to show the more important phenomena to one of my classes.

Thanks to the sensitive and reliable amalgam cells devised by Elster and Geitel,[1] it is an easy matter to show to an audience the more recent experiments bearing upon the phenomenon. The effect of ordinary sources of light, such as the gas flame, is readily shown by these cells, and no difficulty is experienced in determining the relative effect of different wave-lengths by moving the cell through the visible and ultra-violet spectrum formed by a prism.

In discussing the influence of ultra-violet light it is natural, however, to go back first of all to the original observations which led to the discovery of the phenomenon by Hertz. It is possible to show these experiments to three or four persons without any special precautions; but I experienced considerable difficulty in making them visible at a distance. This was particularly true in the case of the first experiments of Hertz, where it was found that the maximum sparking distance of a resonator or induction coil is increased by illuminating the terminals with ultra-violet light. The trouble is that the spark gap must be quite short in order to be sensitive, and this in itself makes it difficult to see the sparks at any great distance. The light used in illuminating the terminals is also troublesome, since it has a tendency to make the spark itself appear less bright by contrast. With the arc light, which forms by far the best and most convenient source for these experiments, I found it almost impossible to make the effect of illumination distinctly visible to more than fifteen or twenty at once. The following method of showing the fundamental phenomena has the advantage of enabling the experiments to be readily seen by a large audience.

To show the influence of the ultra-violet light in increasing the maximum sparking distance the arrangement of apparatus shown in Fig. 1 was used. In series with the spark gap, A, which is to be subjected to

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[1] Wiedemann's Annalen, Vol. 42, p. 564. Such cells may be obtained in a variety of forms from L. Müller-Unkel in Braunschweig.

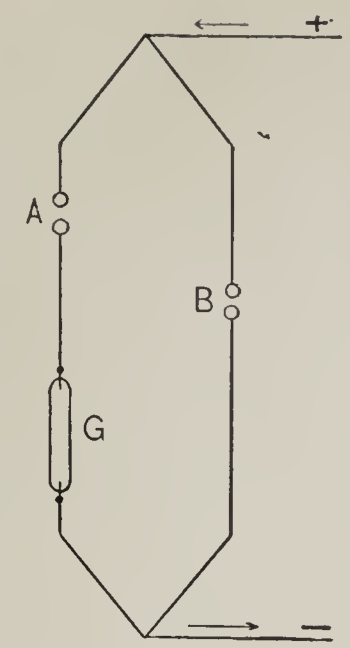

Fig. 1.

the influence of ultra-violet rays, is placed a Geissler tube G. In multiple with the spark gap and tube is placed a second spark gap, B, which is screened from the action of the rays. This second spark gap should be capable of rather delicate adjustment. By altering its length we may cause the spark to pass at B instead of at A, and yet have the difference in the resistance of the two paths so slight that when ultra-violet light is allowed to fall upon A sparks will immediately pass through that branch of the circuit rather than through B. The Geissler tube will then be illuminated each time a spark passes at A. The brilliancy of the Geissler tube discharge is so much greater than that of the small spark that it can be seen at a very considerable distance. A comparatively simple arrangement of screens is sufficient to protect the eye of the observer from the arc light used in illuminating the spark gap.

The arrangement of apparatus above described is especially convenient in showing the opacity or transparency of various substances for the active rays. Paper, mica and glass—in fact, almost all substances—screen off the effect of the arc completely, while quartz and gypsum are fairly transparent. If a spectrum is formed by means of a quartz prism the active rays are found to lie far beyond the limits of the visible region.

This method of showing the effect is not so direct as that used originally by Hertz, and for that reason is perhaps not so much to be commended. But when the experiments are to be shown to a large audience the gain in certainty and ease of operation is so great as to make up for this disadvantage.

The experiments of Wiedeman and Ebert[1] which immediately followed those of Hertz, brought out several points regarding the effect of ultra-violet light whose importance has perhaps not been fully recognized. Hertz's experiments showed that when a spark gap was just a little too long to allow sparks to pass, the illumination of the terminals by ultra-violet light would cause sparking to begin. It was not unreasonable to think that the effect of the ultra-violet rays was merely to overcome the slight opposition to the *beginning* of a discharge, and that when sparking had once begun it would continue without further outside help. The experiments of Wiedemann and Ebert showed, however, that the effect of illumination was to diminish the effective resistance of the spark gap whether a discharge was taking place or not. A convenient method of showing this effect is the following:

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[1] Wiedemann's Annalen, Vol. 33, p. 240. See also *Science*, Vol. IV., No. 102, where an account is given of the work done in investigating the effect of light upon the discharge of electrified bodies up to the close of the year 1896.

A Geissler tube connected in series with the spark gap is caused to rotate by any of the numerous devices used for this purpose. A little motor, which will keep the speed of rotation approximately constant without attention on the part of the lecturer, is to be recommended. If sparks from a Holtz machine are caused to pass at the spark gap, the rotating Geissler tube will show each discharge as a sharply defined spoke in a luminous wheel. The effect of illuminating the spark gap is now to increase the frequency of sparking. Since the effective resistance offered by the spark gap has been diminished, a discharge will take place at a lower difference of potential than before. In order to build up this smaller potential difference a shorter time is required; so that the sparks will follow each other much more rapidly when the spark gap is illuminated than when it is kept in the dark. This is shown in the Geissler tube by the fact that the spokes in the luminous wheel become much more numerous. The change is a very striking one, and with well polished terminals at the spark gap is perfectly definite and certain. So long as no ultra-violet light falls upon the gap the rotating Geissler tube shows a luminous wheel with rather widely separated spokes. On removing a screen so as to allow rays from an arc light to fall upon the gap, the number of spokes will be increased two or three times. In some cases, where the adjustment is just right, the number of spokes will be increased so greatly that the discharge through the tube appears to be continuous, as though the tube were connected directly with the machine. The change in the appearance of the Geissler tube is accompanied by a change in the sound produced by the sparks. But this latter effect can hardly be observed at a distance.

The experiments described above, while extremely easy and certain, require careful attention to some essential points. In the first place the spark gap must be short. The best length seems to be between 0.5 mm. and 1 mm. For a length of spark greater than 2 mm. I have been unable to detect any influence at all due to illumination. The reason for this is not far to seek when we remember that the effect of the ultra-violet rays is merely to facilitate the discharge of *negative* electricity. No action is produced either at the positive terminal or in the space between the terminals. The effect of illumination seems to be equivalent to reducing the length of the air gap by a certain constant amount. When the spark is short the change in the effective resistance will therefore be a large fraction of the whole resistance; while if the spark is long the effect of illumination will be relatively insignificant.

As terminals for the spark gap, I have found brass knobs about half a centimeter in diameter to be most satisfactory. They should be polished frequently, even when there is no sign of deterioration on the surface; merely to rub the surface for a few moments with fine emery paper and oil will sometimes cause the experiment to work successfully when all other adjustments have failed. In the second experiment described above it is necessary to use a Holtz machine rather than an induction coil. In fact I have found the Holtz machine in all cases more convenient for these experiments.

The distance between the spark gap and the arc should not exceed a meter for perfectly certain results, although some effect may be obtained at a greater distance under favorable conditions. It is perhaps unnecessary to add that glass lenses can not be used in connection with the arc. A thin sheet of glass forms just as effective a screen as a sheet of metal.

THE ELECTROSTATIC CAPACITY OF A TWO-WIRE CABLE.

By Geo. W. Patterson, Jr.[1]

MY reason for preparing this paper was the fact that I had found no treatise or text-book on electricity which gave an expression for the electrostatic capacity of a two-wire cable. Since preparing it I have found in Webster's recent book "The Theory of Electricity and Magnetism" an expression for the capacity of excentric cylinders from which my final formula might be deduced by one who is familiar with transformations from electrostatic to practical electromagnetic units. Something analogous is given by Heaviside in his electrical papers, but it is not quite to the point which I sought.

The electrostatic capacity of cables is a matter of great importance in telephony. The distance over which the telephone can be used, other things being the same, is dependent practically on the product of resistance and capacity. For this reason it is desirable that the practical engineer should understand the effect of dimensions and materials in determining the capacity of a particular cable. I shall briefly derive the expression for the capacity of two concentric cylindrical conductors and from that pass on to that of two equal and parallel cylindrical conductors.

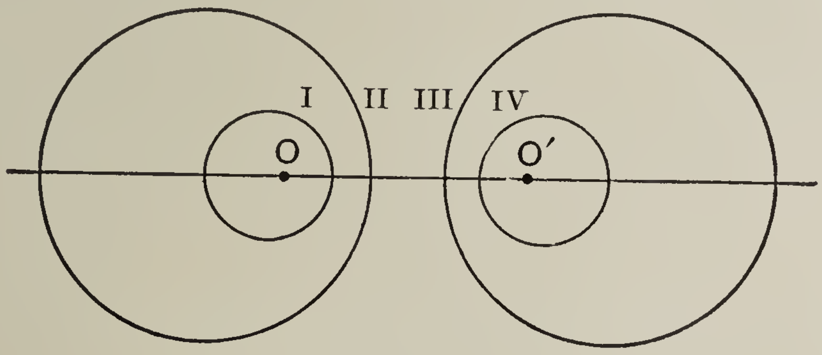

Fig. 1.

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[1] Read at the Detroit Meeting of the A.A.A.S., August, 1897.

Let us consider a system of coaxial cyclinders a, b,. . .n, of indefinite length, and ranging in size from a of small radius r_a, to n of large radius r_n. Suppose a to be a conductor charged with m units of electricity per unit of length. The surface of each cylinder is an equipotential surface of the system. To determine the electric intensity (force on unit charge) at any point distant r from the axis, a well known property of equipotential surfaces may be used. The equipotential surface passing through the point may be supposed to be replaced by a conducting surface in communication with the inner conductor. The electric charge will spread over this surface, giving it a surface density σ equal to $m \div 2\pi r$. The electric intensity at the point is $4\pi\sigma \div K = 2m \div rK$, where K is the dielectric constant of the medium. The difference of potential V between any two equipotential surfaces is the line integral between them of the electric intensity. This line integral, while the same for any such line, is most easily computed along a radius of the system. Its value is

$$\int_a^n \frac{2m\,dr}{Kr} = \frac{2m}{K} \text{ Nap. log. } \frac{r_n}{r_a} = V \tag{1}$$

and the capacity of C per unit of length of a condenser having a and n for its plates, is

$$C = \frac{m}{V} = \frac{K}{2 \text{ Nap. log. } \frac{r_n}{r_a}} \tag{2}$$

This result is in *C. G. S.* electrostatic units. It is more convenient to use common logarithms and the practical electro-magnetic unit, the microfarad. For which purpose (2) must be multiplied by the modulus of the common logarithms and be divided by the square of the velocity of light, and allowance made for the microfarad being 10^{-15} of the absolute unit.

$$\text{Then } C = \frac{0.4343\,K}{18 \times 10^5 \log. \frac{r_n}{r_a}} = \frac{2413 \times 10^{-10} K}{\log. \frac{r_n}{r_a}} \tag{3}$$

Passing now to the case of two equal parallel cylindrical conductors of very small radius, at O and O' of the figure, charged with equal and opposite charges $+m$ and $-m$ per unit of length, it may be seen by a similar course of reasoning that the difference of potential V between D, a point midway between them, and any point distant r' from the axis of the first and r'' from the axis of the second cylinder, is the sum of two expressions similar to (1):

$$\frac{2m}{K} \text{ Nap. log. } \frac{r_d}{r'} \text{ and } \frac{-2m}{K} \text{ Nap. log. } \frac{r_d}{r''}$$

where r_d is the distance from the axis of each to the point D. This sum is

$$\frac{2m}{K} \text{ Nap. log. } \frac{r''}{r'}$$

The locus of all the points when the ratio $\frac{r''}{r'}$ is constant, is a third cylinder, whose axis lies in the plane of the axes of the first and second, and further away from D. *I, II, III* and *IV* are traces of four cylinders corresponding to different values of the ratio $\frac{r''}{r'}$. Each of these cylinders is an equpotential surface of the system and its radius is a mean proportional between the distances from its centre to the lines O and O'. Suppose now that cylinders *II* and *III* are parallel conductors of equal radii R, surrounded by a dielectric whose constant is K. Let r be the smallest value of r' and let d be the least thickness of the dielectric between them. By geometry,

$$r = \frac{1}{2}\sqrt{4Rd + d^2} - \frac{d}{2}.$$

The corresponding value of

$$r'' \text{ is } \frac{1}{2}\sqrt{4Rd + d^2} + \frac{d}{2}.$$

The difference of potential V, between the point D and the surfaces of *II* and *III* is

$$\frac{2m}{K} \text{ Nap. log. } \frac{r''}{r'}$$

and that between *II* and *III* is double this:

$$\frac{4m}{K} \text{ Nap. log. } \frac{r''}{r'} = \frac{4m}{K} \text{ Nap. log. } \frac{\sqrt{4Rd + d^2} + d}{\sqrt{4Rd + d^2} - d}$$

The capacity C per unit of length of a condenser having *II* and *III* as its plates, is

$$\frac{K}{4 \text{ Nap. log. } \dfrac{\sqrt{4Rd + d^2} + d}{\sqrt{4Rd + d^2} - d}}$$

in electrostatic units. Changing to microfarads and common logarithms this becomes:

$$\frac{1206 \times 10^{-10} K}{\log. \dfrac{\sqrt{4Rd + d^2} + d}{\sqrt{4Rd + d^2} - d}}$$

microfarads per centimeter length, or,

$$\frac{1206 \times 10^{-5} K}{\log. \dfrac{\sqrt{4Rd + d^2} + d}{\sqrt{4Rd + d^2} - d}}$$

microfarads per kilometer.

In practice the two conductors are twisted together, and the dielectric about them, moreover, is not of indefinite extent, which causes slight, but negligible changes in the formula.

EXAMPLE.—Suppose the two wires to have diameters equal to the distance between them, then the capacity in microfarads per kilometer will be

$$\frac{1206 \times 10^{-5} K}{\log. \dfrac{\sqrt{5} + 1}{\sqrt{5} - 1}} = 0.0282\, K,$$

or per mile 0.0454 K. For air insulation K is 1. For paraffine it is about 2, and for rubber about 2.25. In some telephone cables, using air and paper strips as insulation, a value of 0.05 microfarads per mile has been obtained with the above dimensions.

In case the two conductors are of unequal radii the calculation becomes somewhat more complicated, but as the case is of relatively little importance it will be sufficient if the method of procedure is merely indicated. Let us take as the conductors whose electrostatic capacity is sought, cylinders I and III of the figure. Call their radii R_1 and R_3 and let D be the least thickness of the dielectric between them. Let r_1 be the distance, along the line of centres, of the image O of cylinder I from its surface, and r_3 the corresponding quantity for cylinder III. The difference of potential between I and III for unit positive and negative charges respectively per unit of length is

$$V = \frac{2}{K} \text{ Nap. log. } \frac{(D + r_1)(D + r_3)}{r_1 r_3}$$

and the capacity of the system per unit of length is,

$$C = \frac{K}{2 \text{ Nap. log. } \dfrac{(D + r_1)(D + r_3)}{r_1 r_3}}$$

In practical units and common logarithms this becomes

$$C = \frac{2412 \times 10^{-5} K}{\log. \dfrac{(D + r_1)(D + r_3)}{r_1 r_3}}$$

microfarads per kilometer.

As O and O' are inverse points with reference to each cylinder we have the relations

$$r_1 = \frac{R_1 (D + r_3)}{D + R_1 + r_3} \text{ and}$$

$$r_3 = \frac{R_3 (D + r_1)}{D + R_3 + r_1}$$

From these equations the values of r_1 and r_3 may be determined as functions of R_1, R_3 and D; and by substitution in the previous equation, the value of C may be determined. The resulting expression is quite long and complicated when expressed in functions of R_1, R_3 and D; but in any particular case, when definite numerical values are assumed, the calculation presents no difficulties.

NEW BOOKS.

The Phase Rule. By WILDER D. BANCROFT. 8vo, pp. 255. The Journal of Physical Chemistry, Ithaca, N. Y., 1897.

In the preface the author suggests as natural divisions of physical chemistry, Qualitative Equilibrium, Quantitative Equilibrium, Electrochemistry, and Mathematical Theory. This classification seems arbitrary and the wisdom of presenting a subject so quantitative in its nature and development with as little mention as possible of the quantitative side, is questionable; yet, by this very method of treatment much is gained in the way of simplicity and singleness of point of view.

The Gibb's Phase Rule furnishes the basis of classification, systems of one, two, three and four components being taken up in order. The simpler cases, starting with water in its three phases, solid, liquid and vapor, thus receive attention first, and the advance to the more complicated systems is gradual.

The work has been well planned and well executed. We are given a very complete presentation and a full critical discussion of the phenomena involved. Much of the experimental matter has been published before except in the original articles, the references to which are a valuable feature of this treatise.

Some of the views advanced are subject to criticism; for example, the distinction between solvent and solute in the liquid phase of two components, and some of the criticisms of the work of other authors. But the author is in general careful to distinguish between theories well established by experiment, and those that have no such support.

The book is a valuable addition to the literature of physical chemistry and should be read by all who are engaged in this line of work. The suggestion of many interesting problems, which have not yet received experimental attention, should serve as a stimulus to much fruitful investigation.

CLARENCE McC. GORDON.

Practical Electrical Measurements. By E. H. CRAPPER. 8vo, pp. 125. London, Whittaker & Co., 1897.

This small laboratory manual is intended for first year students in English technical schools and it will serve its purpose very well. While perhaps too large a part of the book is taken up by the study of the action of a magnet upon another, of the tangent galvanometer and the numerous exercises connected with the latter, there are many points which will recommend the use of the book. So the experiments in the chapter on "Electromagnetism," showing the nature of the magnetic field set up by a current, are excellent and stand in a striking contrast to those given in our corresponding American books, in which too little stress is laid upon the correct conception of the magnetic field. The platting of the lines of magnetic force around a straight wire and a solenoid, carrying a current, should lead up to similar experiments with magnets rather than the reverse, and they will explain the modern idea of poles, magnetization, etc., a great deal better than the usual elementary exercises given under magnetism. Work of this kind may be carried even farther. For instance, a model representing a D'Arsonval galvanometer is easily constructed and shows in a very clear manner the action of the instrument.

Another good feature of Mr. Crapper's book are the exercises given as an appendix to many of the practical experiments. They will teach the student to think, deepen his interest in the experiment and draw his attention to the accuracy obtainable by the method he has just applied.

The number of specific electrical experiments seems rather small in comparison with that devoted to magnetism and no work with the condenser is given, but it may be that this is reserved for the second volume, dealing with "Practical Electrical Engineering," a book which the author has in preparation as a sequel to the present one.

K. E. GUTHE.

The Localization of Faults in Electric Light Mains. By F. CHARLES RAPHAEL. Octavo. pp. 184. New York, The D. Van Nostrand Company. London, The Electrician Publishing Company, 1896.

The author of this useful book states that although the localization of faults in telegraph cables has been dealt with fully in several handbooks, the treatment of faulty electric light and power cables has never been discussed in an equally comprehensive manner. The methods of treatment of the two cases necessarily differ, inasmuch as faults in telegraph cables are rarely localized before their resistance has become low as compared with the resistance of the cable itself, while the contrary is more usually the case in electric light mains. The book is confined to this one subject and thus differs from the more general works on electrical testing. The author has given an adequate and rather complete treatment of his subject. The book is a practical one, intended as a handbook for central station engineers; yet it has brought together so much that is instructive that it may well be given a place as a student's reference book in electrical laboratories.

The author divides "faults" into three classes: (1) Discontinuity of the conductor; (2) Short circuits; (3) Electrical contact between the conductor and the earth. The major portion of the book deals with faults of the third class.

The theory of the methods employed and the mathematical work required therefor is omitted from the body of the book and is incorporated in a concluding chapter.

The opening chapter is introductory and descriptive of galvanometers, switches and various testing instruments employed. Chapters II. and III. pertain to insulation testing during working of low and high pressure mains, respectively, a subject quite germane to that of localizing faults. The treatment of the insulation testing of low pressure mains is complete, but we find the few pages on testing of high pressure mains rather inadequate. The main portion of the book is divided into chapters on "General Methods of Fault Localizing," "Localizing during Working" and "Fault Signalling Networks."

The author has performed a service in collecting together material hitherto widely scattered and placing it in systematic order. The author has added some new material, *e. g.*, a discussion of how low must the resistance of a fault be to be localized to a given degree of accuracy. As a result of this investigation, he determines a formula for the possible error (in yards) in localizing a fault under given conditions.

The concluding chapter on "Algebra," which contains all the mathematical work of the book, although good as far as it goes, is not complete. Unfortunately, too, the author does not refer in the body of the book to the proofs given in this concluding chapter. The incompleteness of the theoretical treatment weakens the book in its use as other than a practical manual. The valuable references which the author has given to original sources might be more complete.

The typography and illustrations are only fair. Many of the diagrams have been prepared especially for the book; the illustrations of apparatus are taken chiefly from trade catalogues.

FREDERICK BEDELL.

An Introductory Course in Quantitative Chemical Analysis. By PERCY NORTON EVANS. 12mo, pp. iv + 81. Ginn & Company, Boston, 1897. (*Received.*)

The Commercial Uses of Coal Gas. By THOS. FLETCHER. Pp. 104. Fletcher, Russell & Co., Warrington, Manchester and London. 1897. (*Received.*)

Elements of Geometry. By ANDREW W. PHILLIPS and IRVING FISHER. viii and 540 pages. Harper & Brothers, New York, 1897. (*Received.*)

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